SCIENCE

FRIDAY, SEPTEMBER 30, 1910

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE FLORA OF THE BRITISH ISLANDS[1]

THE honor conferred in the election to be president for the year of the Botanical Section of the British Association imposes the duty of preparing an address. I trust that my selection of a subject will not be attributed by any one to a want of appreciation of the worth and importance of certain sides of botanical research to which I shall have less occasion to refer. These have been eloquently supported by former presidents, and I take this opportunity to express the thanks I owe for the benefit received from their contributions to the advancement of the science of botany. They have told us of the advance in departments of which they could speak as leaders in research, and I do not venture to follow in their steps. My subject is from a field in which I have often experienced the hindrances of which I shall have to speak, both in personal work and still more as a teacher of students, familiar with the many difficulties that impede the path of those who would gladly give of their best, but find the difficulties for a time almost insurmountable, and who are too frequently unable to spare the time or labor to allow of their undertaking scientific investigations that they might well accomplish, and in which they would find keen pleasure under other conditions. Those whose tastes lie in the direction of studying plants in the field rather than in the laboratory are apt to find themselves hampered seriously if they seek to become acquainted with the plants of their own

[1] Address of the president of the Botanical Section of the British Association for the Advancement of Science, Sheffield, 1910.

vicinity; and, if they wish to undertake investigations in the hope of doing what they can to advance botanical science, they may find it scarcely possible to ascertain what has been already done and recorded by others.

For a time the knowledge of plants was too much confined to the ability to name them according to the system in vogue and to a knowledge of their uses, real or imagined. The undue importance attached to this side of the study, even by so great a leader as Linnæus, naturally led to a reaction as the value of other aspects of botany came to be realized, and as improvements in the instruments and methods of research opened up new fields of study. The science has gained much by the reaction; but there is danger of swinging to the other extreme and of failing to recognize the need to become well acquainted with plants in their natural surroundings. The opportunities for study in the laboratory are so great and so much more under control, and the materials are so abundant and of so much interest, that there is for many botanists a temptation to limit themselves to such work, or at least to regard work in the field as subordinate to it and of little value. It is scarcely necessary to point out that each side is insufficient alone. Yet some find more pleasure in the one side, and do well to make it their chief study; while they should recognize the value of the other also, and learn from it.

It is especially on behalf of the work in the field that I now wish to plead. There are few paths more likely to prove attractive to most students. The study of the plants in their natural environments will lead to an understanding of their nature as living beings, of their relations to one another and to other environments, of the stimuli to which they respond, and of the struggle for existence that results in the survival of certain forms and the disappearance of others. In this way also will be gained a conception of the true meaning and place of classification as an indispensable instrument for accurate determination and record, and not as an end in itself. To one that has once gained a true insight into the pleasure and worth of such studies, collections made for the sake of mere possession or lists of species discovered in a locality will not suffice. Many questions will arise which will prove a constant source of new interest. From such studies a deep and growing love for botany has in not a few cases arisen.

The British flora has interested me for upwards of forty years, and has occupied much of my attention during that time—not only as desirous to aid by my own efforts to extend our knowledge of it, but also, as a teacher, seeking to assist my students to become able to do their parts also, and making use of the materials within reach to enable me to help them. Thus our present knowledge of the plants of our own country has become known to me, and the difficulties of acquiring that knowledge have also become known through both my own experience and those of my students. The nature of the hindrance and difficulties that at present bar the way has also become familiar, as well as the steps to be taken to clear some of them away and to make the path less difficult to those who come after us; and I have also gained a fairly good acquaintance with the means at the command of students of the floras of other countries, so as to have a standard for comparison in the estimate to be formed of the condition of matters in our own country.

In how far is the present provision for the study of the flora of the British Islands sufficient and satisfactory?

I venture to hope that the subject will be

regarded as among those for the consideration of which the British Association was formed, and that a favorable view will be taken of the conclusions which I take this opportunity to lay before you. What, then, is the present provision for the study of our plants? Since the days of Morrison and Ray there have been many workers, especially during the past century; and an extensive literature has grown up, in the form both of book and of papers, the latter more or less comprehensive, in the scientific journals and in the transactions of societies. These papers contain much that is of great value; but, owing to the absence of any classified index, most of the information in it is beyond the reach of any one, except at the expenditure of much time and labor. The constantly increasing accumulation of new publications makes the need for a classified index always more urgent; for the mass of literature is at present one of the greatest obstacles to the undertaking of new investigations because of the uncertainty whether they may not have been already undertaken and overlooked through want of time or opportunity to search the mass exhaustively.

While the early writers of descriptive floras sought to include every species of plant known to occur in Britain, this has not been attempted during the past seventy or eighty years, and instead of one great work we know have monographs of the greater groups, such as Babington's "Manual" and Hooker's "Student's Flora" of the vascular plants, Braithwaite's "Moss-flora," etc. Local floras still, in a good many cases, aim at including all plants known to grow apparently wild in the districts to which they refer; but they are often little more than lists of species and varieties and of localities in which these have been found. In some, however, there are descriptions of new forms and notes of general value, which are apt to be overlooked because of the place in which they appear.

The early works were necessarily not critical in their treatment of closely allied species and varieties, but they are valuable as giving evidence of what plants were supposed to be native in England when they were published. Even the works that were issued after Linnæus had established the binominal nomenclature for a time related almost wholly to England. Sibbald in "Scotia Illustrata" (1684) enumerated the plants believed by him to be native in Scotland, and of those then cultivated. Between his book and Lightfoot's "Flora Scotia," published in 1777, very little relating to the flora of Scotland appeared. Irish plants were still later in being carefully studied.

The floras of Hudson, Withering, Lightfoot and Smith, all of which include all species of known British plants, follow the Linnæan classification and nomenclature in so far as the authors were able to identify the Linnæan species in the British flora. "English Botany," begun in 1795, with plates by Sowerby and text by Smith, was a work of the first rank in its aim of figuring all British plants and in the excellence of the plates; but it shared the defect of certain other great floras in the plates being prepared and issued as the plants could be procured, and thus being without order. Its cost also necessarily put it beyond the reach of most botanists, except those that had the advantage of access to it in some large library. A second edition, issued at a lower price, and with the plants arranged on the Linnæan system, was inferior to the first, in the plates being only partially colored and in having the text much curtailed. The so-called third edition of the "English Botany," issued 1868–86, is a new work as far as the text is concerned, that being the

work of Dr. Boswell Syme, who made it worthily representative of its subject; but the plates, with few exceptions, are reissues of those of the first edition, less perfect as impressions and far less carefully colored; and this applies with still greater force to a reissue of the third edition a few years ago. This edition, moreover, included only the vascular plants and Characeæ. As this is the only large and fully illustrated British flora that has been attempted, it is almost needless to add that in this respect provision for the study of the flora of our islands is far behind that of certain other countries, and very notably behind that made in the "Flora danica."

Turning next to the provision of less costly aids to the study of British plants, we have manuals of most of the larger groups. The vascular plants are treated of in numerous works, including a considerable number of illustrated books in recent years, inexpensive but insufficient for any but the most elementary students. Fitch's outline illustrations to Bentham's "Handbook to the British Flora," supplemented by W. G. Smith, were issued in a separate volume in 1887, which is still the best for use in the inexpensive works of this kind. Babington's "Manual," on its first appearance in 1843, was gladly welcomed as embodying the result of careful and continued researches by its author into the relations of British plants to their nearest relatives on the continent of Europe; and each successive issue up to the eighth in 1881 received the careful revision of the author, and contained additions and modifications. In 1904 a ninth edition was edited, after the author's death, by H. and J. Groves; but, though the editors included notes left by Professor Babington prepared for a new edition, they were "unable to make alterations in the treatment of some of the critical genera which might perhaps have been desirable." The "Student's Flora of the British Islands," by Sir J. D. Hooker, issued in 1870, took the place of the well-known "British Flora" (1830, and in subsequent editions until the eighth in 1860, the last three being issued in collaboration by Sir W. J. Hooker and Professor Walker Arnott). The third edition of the "Student's Flora" appeared in 1884, and there has been none since. Mr. F. N. Williams's "Prodromus Floræ Britannicæ," begun in 1901, of which less than one half has yet appeared, though a work of much value and authority, is scarcely calculated for the assistance of the ordinary student; and Mr. Druce's new edition of Hayward's "Botanist's Pocket Book" "is intended merely to enable the botanist in the field to name his specimens approximately, and to refresh the memory of the more advanced worker." In all the books that are intended for the use of British botanists, apart from one or two recently issued local floras, the classification is still that in use in the middle of last century, even to the extent in the most of them of retaining Coniferæ as a division of Dicotyledones. Apart from this, the critical study of British plants has led to the detection of numerous previously unobserved and unnamed forms, which find no place in the "Student's Flora," and are only in part noticed in the recent edition of the "Manual."

The "Lists" of vascular plants of the British flora that have recently been issued by Messrs. Rendle and Britten, by Mr. Druce, and as the tenth edition of the "London Catalogue of British Plants" are all important documents for the study of the British flora; but they illustrate very forcibly certain of the difficulties that beset the path of the student eager to gain a knowledge of the plants of his native land. In these lists he finds it scarcely possible to

gain a clear idea of how far the species and varieties of the one correspond with those of the other, owing to the diversities of the names employed. It would be a great boon to others as well as to students were a full synonymic list prepared to show clearly the equivalence of the names where those for the same species or variety differ in the different lists and manuals. Probably in time an agreement will be generally arrived at regarding the names to be accepted, but that desirable consummation seems hardly yet in sight. Meantime the most useful step seems to be to show in how far there is agreement in fact under the different names.

Among the cryptogams certain groups have fared better than the higher plants as regards both their later treatment and their more adequate illustration by modern methods and standards. Several works of great value have dealt with the mosses, the latest being Braithwaite's "British Moss-flora," completed in 1899. The Sphagna were also treated by Braithwaite in 1880, and are to be the subject of a monograph in the Ray Society's series. The liverworts have been the subject also of several monographs, of which Pearson's is the fullest.

Among the Thallophyta certain groups have been more satisfactorily treated than others—*e. g.*, the Discomycetes, the Uredineæ and Ustilagineæ, the Myxomycetes and certain others among the fungi, and the Desmidiaceæ among the algæ; but the Thallophyta as a whole are much in need of thorough revision to place them on a footing either satisfactory or comparable to their treatment in other countries.

Of the Thallophyta many more of the smaller species will probably be discovered within our islands when close search is made, if we may judge by the much more numerous forms already recorded in certain groups abroad, and which almost certainly exist here also; but among the higher plants it is not likely that many additional species will be discovered as native, yet even among these some will probably be found. It is, however, rather in the direction of fuller investigation of the distribution and tendencies to variation within our islands that results of interest are likely to be obtained.

The labors of H. C. Watson gave a very great stimulus to the study of the distribution of the flora in England and Scotland, and the work he set on foot has been taken up and much extended by numerous botanists in all parts of the British Islands. It is largely owing to such work and to the critical study of the flora necessary for its prosecution that so many additions have been made to the forms previously known as British. Many local works have been issued in recent years, often on a very high standard of excellence. Besides these larger works scientific periodicals and transactions of field clubs and other societies teem with records, some of them very brief, while others are of such size and compass that they might have been issued as separate books. A few of both the books and papers are little more than mere lists of names of species and varieties observed in a locality during a brief visit; but usually there is an attempt at least to distinguish the native or well-established aliens from the mere casuals, if these are mentioned at all. In respect of aliens or plants that owe their presence in a district to man's aid, intentional or involuntary, their treatment is on no settled basis. Every flora admits without question species that are certainly of alien origin, even such weeds of cultivated ground as disappear when cultivation is given up, as may be verified in too many localities in some parts of our country. Yet other species

are not admitted, though they may be met with here and there well established, and at least as likely to perpetuate their species in the new home as are some native species.

Comparatively few writers seek to analyze the floras of the districts treated of with a view to determine whence each species came and how, its relation to man, whether assisted by him in its arrival directly or indirectly, whether favored or harmfully affected by him, its relations to its environment—especially to other species of plants and to animals, and other questions that suggest themselves when such inquiries are entered on. It is very desirable that a careful and exhaustive revision of the British flora should be made on these and similar lines. In such a revision it is not less desirable that each species should be represented by a good series of specimens, and that these should be compared with similar series from other localities within our islands, and from those countries from which it is believed that the species originally was sprung. Such careful comparison would probably supply important evidence of forms being evolved in the new environments, differing to a recognizable degree from the ancestral types, and tending to become more marked in the more distant and longer isolated localities. An excellent example of this is afforded by the productive results of the very careful investigation of the Shetland flora by the late Mr. W. H. Beeby.

Within recent years excellent work has been done in the study of plant associations, but the reports on these studies are dispersed in various journals (often not botanical), and are apt to be overlooked by, or to remain unknown to, many to whom they would be helpful. The same is true in large measure of the very valuable reports of work done on plant-remains from peat-mosses, from lake deposits, and from other recent geological formations, researches that have cast such light on the past history of many species as British plants, and have proved their long abode in this country. Mr. Clement Reid's "Origin of the British Flora," though published in 1899, has already (by the work of himself and others) been largely added to, and the rate of progress is likely to become still more rapid. Among the fruits and seeds recorded from interglacial and even from preglacial deposits are some whose presence could scarcely have been anticipated, *e. g., Hypecoum procumbens,* in Suffolk. Some of the colonists, or aliens now almost confined to ground under cultivation, have been recorded from deposits that suggest an early immigration into the British Islands. While much remains to be discovered, it is desirable that what is already established should find a place in the manuals of British botany.

Apart from the descriptive and topographical works and papers on our flora, there is a serious lack of information gained from the study of our British plants. Although a few types have received fuller study, we have little to compare with the work done in other countries on the structure and histology of our plants, on the effects of environment, on their relations to other species and to animals, and on other aspects of the science to which attention should be directed. On these matters, as on a good many others, we gain most of what information can be had not from British sources, but from the literature of other countries, though it is not wise to assume that what is true elsewhere is equally true here. It is as well, perhaps, that for the present such subjects should find scanty reference in

the manuals in ordinary use; but, when trustworthy information has been gained within the British Islands, under the conditions prevailing here, these topics should certainly not be passed over in silence. Students of the British flora have as yet no such works of reference as Raunkjaer's book on the Monocotyledons of Denmark or the admirable "Lebensgeschichte der Blütenpflanzen Mitteleuropas," at present being issued by Drs. Kirchner, Loew and Schröter.

In a complete survey of the British botany there must be included the successive floras of the earlier geological formations, though they can not as yet be brought into correlation with the recent or existing floras. In the brilliant progress made recently in this field of study our country and the British Association are worthily represented.

The present provision for the study of the British flora and the means that should be made use of for its extension appear to be these:

Much excellent work has already been accomplished and put on record towards the investigation of the flora, but much of that store of information is in danger of being overlooked and forgotten or lost, owing to the absence of means to direct attention to where it may be found. A careful revision of what has been done and a systematic subject-index to its stores are urgently required.

The systematic works treating of the flora are in great part not fully representative of the knowledge already possessed, and require to be brought up to date or to be replaced by others.

Great difficulty is caused by the absence of an authoritative synonymic list that would show as far as possible the equivalence of the names employed in the various manuals and lists. There is much reason to wish that uniformity in the use of names of species and varieties should be arrived at, and a representative committee might assist to that end; but, in the meantime, a good synonymic list would be a most helpful step towards relieving a very pressing obstacle to progress.

There is need for a careful analysis of the flora with a view to determining those species that owe their presence here to man's aid, intentional or unconscious; and the inquiry should be directed to ascertain the periods and methods of introduction, any tendencies to become modified in their new homes, their subsequent relations with man, and their influence on the native flora, whether direct or by modifying habitats, as shown by *Lupinus nootkatensis* in the valleys of rivers in Scotland.

Those species that there is reason to regard as not having been introduced by man should be investigated as regards their probable origins and the periods and methods of immigration, evidence from fossil deposits of the period during which they have existed in this country, their constancy or liability to show change during this period, their resemblance to or differences from the types in the countries from which they are believed to have been derived, or the likelihood of their having originated by mutation or by slow change within the British Islands, and their relation to man's influence on them (usually harmful, but occasionally helpful) as affecting their distribution and permanence.

The topographical distribution, though so much has been done in this field during the past sixty or seventy years, still requires careful investigation, to determine not merely that species have been observed in certain districts, but their relative frequency, their relations to man (natives of one part of our country are often aliens in

other parts), whether increasing or diminishing, altitudes, habitats, etc. From such a careful topographical survey much should be learned of the conditions that favor or hinder the success of species, of the evolution of new forms and their relation to parent types in distribution, especially in the more isolated districts and islands, and of other biological problems of great interest. A most useful aid towards the preparation of topographical records would be afforded by the issue at a small price of outline maps so as to allow of a separate map being employed for recording the distribution of each form.

A careful study of the flora is also required from the standpoint of structure and development, with comparison of the results obtained here with those of workers in other countries where the same or closely allied species and varieties occur. It is also needed in respect of the relations between the plants and animals of our islands, both as observed here and in comparison with the already extensive records of a similar kind in other countries. On such topics as pollination, distribution of seeds, and injuries inflicted by animals and galls produced by animals or plants we have still to make use very largely of the information gained abroad; and the same holds good with regard to the diseases of plants.

While "English Botany" in its first edition was deservedly regarded as a work of the first rank among floras, it has long been defective as representing our present knowledge of British plants, and it has not been succeeded by any work of nearly equal rank, while other countries now have their great floras of a type in advance of it. There is need for a great work worthy of our country, amply illustrated so as to show not only the habit of the species and varieties, but also the distinctive characters and the more important biological features of each. Such a flora would probably require to be in the form of monographs by specialists, issued as each could be prepared, but as part of a well-planned whole. It should give for each plant far more than is contained in even the best of our existing British floras. Means of identification must be provided in the description, with emphasized diagnostic characters; but there should also be the necessary synonymy, a summary of topographical distribution, notes on man's influence upon distribution, abundance, etc., on any biological or other point of interest in structure or relations to habitat, environment, associated animals or plants, diseases, etc. Local names, uses, and folklore should also be included; and for this the need is all the greater, because much of such old lore is rapidly being forgotten and tends to be lost. In a national flora there should be included an account of the successive floras of former periods, and, as far as possible, the changes that can be traced in the existing flora from its earliest records to the time of issue should be recorded.

A flora of this kind would not only afford the fullest possible information with regard to the plant world of the British Islands at the date of issue, but would form a standard with which it could be compared at later periods, so as to permit of changes in it being recognized and measured. In the meanwhile the production of such a flora can be regarded only as an aim towards which to press on, but which can not be attained until much has been done. But while the fulfilment must be left to others, we can do something to help it on by trying to remove difficulties from the way, and to bring together materials that may be used in its construction.

I have sought to call attention to the difficulties that I have experienced and to directions in which progress could be made at once, and to provision which should be made for the advancement of the study of the British flora with as little delay as possible. There is, I feel assured, the means of making far more rapid and satisfactory progress towards the goal than has yet been accomplished. Many persons are interested in the subject, and would gladly give their aid if they knew in what way to employ it to the best purpose. As a nation we are apt to trust to individual rather than to combined efforts, and to waste much time and labor in consequence, with discouragement of many who would gladly share the labor in a scheme in which definite parts of the work could be undertaken by them.

I believe that a well-organized botanical survey of the British Islands would give results of great scientific value, and that there is need for it. I believe, also, that means exist to permit of its being carried through. There is no ground to expect that it will be undertaken on the same terms as the Geological Survey. A biological survey must be accomplished by voluntary effort, with possibly some help towards meeting necessary expenses of equipment from funds which are available for assistance in scientific research. Is such a survey not an object fully in accord with the objects for which the British Association exists? In the belief that it is so, I ask you to consider whether such a survey should not be undertaken; and, if you approve the proposal, I further ask that a committee be appointed to report on what steps should be taken towards organizing such a survey, and preparing materials for a national flora of the British Islands.

JAMES W. H. TRAIL

THE AMERICAN CHEMICAL SOCIETY

THE readers of SCIENCE will be interested to know that another gold medal award for chemical research has been established. This medal is to be known as the Willard Gibbs Medal and is founded under the control of the Chicago Section of our society through the generosity of Mr. W. A. Converse, of that city. The rules governing its award as transmitted to me by A. L. Nehls, secretary, are as follows:

I. A gold medal shall be awarded annually for the best paper or address presented before the Chicago Section of the American Chemical Society, provided it be of sufficient merit. This medal shall be known as the Willard Gibbs medal, founded by Wm. A. Converse. The award may be made to any one, provided he be a member of the American Chemical Society at the time the paper or address was delivered, and provided it is eligible under the following conditions:

(*a*) The medal shall be awarded at the November meeting of the Chicago Section for a paper or address delivered before the section between September 1 of the previous year and July 1 of the year of the award. The first medal shall be awarded in November, 1911.

(*b*) The paper or address shall be complete in itself, shall be presented by the author, and shall not have been read or published previously. To be considered for the award a typewritten copy of the paper or address shall be submitted to the jury, through the chairman of the section.

(*c*) It is desired that the paper or address, if suitable, be published in one of the publications of the American Chemical Society.

II. The jury to determine the award of the medal shall consist of the chairman of the Chicago Section at the time the award is made, who shall, ex officio, be chairman of the jury, and four other members of the section duly elected by it.

III. The executive committee of the Chicago Section shall have the power to decide any question not specifically covered by these rules.

IV. The Chicago Section shall have the power to change or amend these rules under

the same conditions and in the same manner as the by-laws of the section.

CHARLES L. PARSONS,
Secretary

SCIENTIFIC NOTES AND NEWS

A BRONZE statue of Lord Kelvin by Mr. Bruce-Joy is to be erected at Belfast.

SIR WILLIAM CHRISTIE, astronomer royal since 1881, is about to retire and will be succeeded by Professor F. W. Dyson, astronomer royal for Scotland.

DR. OSCAR BOLZA, until recently professor of mathematics in the University of Chicago and still honorary professor there, has been appointed honorary professor at Freiburg, where he will hereafter reside.

IT is proposed to present a portrait to the College of Physicians of Dr. James Tyson, who has recently retired from the chair of medicine at the University of Pennsylvania.

GEHEIMRATH F. E. SCHULZE, professor of zoology in the University of Berlin, has celebrated his seventieth birthday. A fine portrait of this eminent man of science has been issued, which will be a source of gratification to his many friends and admirers in America.

DR. FRANZ MERTENS, professor of mathematics at Vienna, and well known for his contributions to the theory of numbers, has celebrated his seventieth birthday.

DR. JOHANN JUSTUS REIN, professor of geography at Bonn, has retired from active service.

DR. J. W. SPENCER has spent the summer in Norway studying certain erosion features.

AN International Congress of Tuberculosis is to be held in Rome next September under the presidency of Professor Guido Baccelli.

DR. HENRY FAIRFIELD OSBORN, president of the American Museum of Natural History, will make the address at the opening of Columbia University, his subject being "Huxley on Education."

THE original laboratory of Liebig in Giessen is to be purchased and preserved as a memorial to the eminent chemist. An anonymous donor has guaranteed 60,000 Marks for this purpose.

A MONUMENT in memory of Dr. Niels Finsen, to whom we owe the light treatment of lupus and other diseases, was recently unveiled at Copenhagen.

Nature states that a granite obelisk erected in the parish churchyard of Forfar to the memory of George Don, the Scottish botanist, was unveiled last week by Mr. G. Claridge Druce, who gave an address on Don's achievements as a botanist.

WILLIAM HARMON NILES, Meredith professor of geology at the Massachusetts Institute of Technology, to which chair he was appointed in 1871, known for his valuable contributions to geology, died on September 13, at the age of seventy-two years.

MR. JOHN LANGTON, formerly Hunterian professor of pathology and surgery at the Royal College of Surgeons, London, died at the age of seventy years.

MR. C. A. BRERETON, a well-known British engineer, has died at the age of fifty-nine years.

DR. F. P. GULLIVER, secretary of Section E —Geology and Geography—American Association for the Advancement of Science, writes that it was impossible to arrange for a summer meeting of Section E at an earlier date than September 15. Between 40 and 50 geologists and geographers had previously expressed their desire to attend such a meeting, but September 15 proved to be too late for many of them, so that it has been decided to give up the meeting for this year. It is hoped, however, that the plans made for this summer meeting at Nantucket and Marthas Vineyard may be carried out at some future time.

THE American Electrochemical Society will hold its next semi-annual meeting in Chicago on October 13, 14, 15.

A REUTER message from Paris states that a private conference of the official delegates of the various governments at the Pure Food Congress has arranged to make certain methods of analysis international, with the consequence that when any food is in future submitted to an analytical test it will have to conform to that international standard.

THE junior mining engineers of the Case School of Applied Science, of Cleveland, Ohio, spent the month of June on a practise term trip through the west. They visited Gary, Chicago, Denver, Golden, Idaho Springs, Georgetown, Colorado Springs, Cripple Creek and Pueblo. Those in charge of the trip were Dr. A. W. Smith, professor of metallurgy, Dr. F. R. Van Horn, professor of geology and mineralogy, and Mr. L. O. Howard, instructor in mining and ore treatment.

THE Bureau of Statistics of the Department of Agriculture reports that the month of August was favorable for crops in general, taking the United States as a whole, the deterioration during the month being about 0.6 per cent., whereas there is an average decline in August of 3.3 per cent. Aggregate crop conditions in the United States on September 1 (or at time of harvest) were about 0.4 per cent. lower than on corresponding date a year ago and 2.8 per cent. lower than the average condition on September 1 (or at time of harvest) of the past ten years. The area under cultivation is about 3.2 per cent. more than last year. By states, the aggregate of crop conditions on September 1 (100 representing the average on September 1 of the past ten years) was as follows: Maine, 111; New Hampshire, 110; Vermont, 113; Massachusetts, 102; Rhode Island, 103; Connecticut, 113; New York, 106; New Jersey, 107; Pennsylvania, 103; Delaware, 110; Maryland, 103; Virginia, 106; West Virginia, 93; North Carolina, 107; South Carolina, 104; Georgia, 100; Florida, 98; Ohio, 98; Indiana, 104; Illinois, 104; Michigan, 97; Wisconsin, 78; Minnesota, 92; Iowa, 97; Missouri, 105; North Dakota, 41; South Dakota, 76; Nebraska, 89; Kansas, 96; Kentucky, 98; Tennessee, 106; Alabama, 106; Mississippi, 106; Louisiana, 100; Texas, 103; Oklahoma, 90; Arkansas, 112; Montana, 80; Wyoming, 103; Colorado, 85; New Mexico, 83; Arizona, 79; Utah, 100; Nevada, 129; Idaho, 92; Washington, 82; Oregon, 106; California, 114. The conditions of various crops in the United States on September 1 (or at time of harvest),—100 representing for each crop, not its normal condition, but its average condition on September 1, or at time of harvest (10-year average for most crops)—was as follows: Peaches (production), 113.1; winter wheat (yield per acre), 110.5; oats, 104.8; cabbages, 104.4; hops, 102.6; rye (yield per acre), 101.8; cranberries, 99.6; cotton, 98.6; corn, 98.4; hemp, 98.3; sweet potatoes, 98.2; sugar cane, 97.6; cantaloupes, 97.3; sorghum, 97.1; oranges, 97.1; watermelons, 97.0; onions, 96.3; tomatoes, 95.5; kaffir corn, 95.0; buckwheat, 94.6; tobacco, 94.4; sugar beets, 94.1; hay (yield per acre), 93.1; alfalfa, 92.9; potatoes, 88.3; grapes, 87.7; millet, 86.6; apples, 85.6; barley, 84.0; spring wheat, 80.9; flaxseed, 55.8. The number of stock hogs in the United States on September 1 is estimated as 100.3 per cent. of the number on September 1, 1909. The acreage of clover for seed is estimated as 116.7 per cent. of last year's acreage.

COPPER was once supposed to occur at only a few places in the United States, but it is now known to be widespread. Most of the deposits are of low grade, but improved modern methods of treatment have made low-grade copper ores very valuable. Geologists of the United States Geological Survey describe the copper deposits of three localities in an advance chapter from the survey's Bulletin 430, containing short papers and preliminary reports on work done in 1909. The Shasta region in California is the second largest copper region in the United States that can be considered a geologic unit. In shape it forms a curved belt 35 miles long, popularly known as the "copper crescent." Copper sulphides have been known to occur with the gold lodes of this region for many years, but were not handled until 1895, and since that year the region has produced 300,000,000 pounds of copper. In 1909 it produced 50,000,000 pounds, which makes it rank as the sixth or seventh copper district in the United States. The ores are pyritic and are of medium richness, averaging 3 to 3½ per cent. Some of them form the largest sulphide ore bodies in the world, measuring 1,200 by 300 by 300 feet. They represent, not the filling of cavities, but the replacement of parts of the rock by which

they are surrounded. The report on these deposits was made by L. C. Graton. In Bear Lake County, Idaho, copper deposits occur near Montpelier. Here however, they are mostly carbonate and not sulphide ores. Their value has not yet been definitely proved, nor is their extent known. The chief project for their development is the Bonanza shaft, which has gone down 350 feet but has not yet shipped ore. Shales, stained green, maroon and chocolate by iron, abound in the region, the colors mimicking those of copper stains and misleading the prospector, who supposes that their vivid tints are indications of copper. The ores run only about 2 per cent. but may be made to pay by proper treatment. The deposits are described by H. S. Gale. Near South Mountain, Pennsylvania, copper in the shape of blebs, grains and wires is associated with ancient lavas, particularly with the greenstone that is so widespread in that region. Traces of copper are found for eight miles, from the Gettysburg pike to a point beyond the Maryland state line. Most of the prospects are at stream crossings, where the overlying rocks have been worn away. The copper was brought up from the interior of the earth with the lava but was then very finely disseminated through the mass and was worthless. Later it was concentrated in veins by hot circulating waters, which dissolved it and later redeposited it on the walls of cavities and in other places. These deposits, which are described by G. W. Stose, have been known for seventy years but have not yet proved to be commercially important. Systematic search, however, might reveal valuable deposits.

UNIVERSITY AND EDUCATIONAL NEWS

Frederick W. Doolittle, B.S. (C.E.) 1907, instructor in civil engineering in the University of Colorado, has been appointed assistant professor of mechanical engineering at the University of Wisconsin.

Dr. E. T. Bell, formerly of the University of Missouri, has begun his duties as assistant professor of anatomy in the University of Minnesota.

Dr. Alexander Petrunkevitch, honorary curator in the American Museum of Natural History, has been appointed instructor in zoology in the Sheffield Scientific School of Yale University.

E. G. Peterson, Ph.D. (Cornell), has been appointed professor of bacteriology in the Oregon Agricultural College. At the same institution Mr. William E. Lawrence has been appointed instructor in botany.

Mr. John E. Gutberlet, assistant in biology at the University of Colorado, has accepted a position in the biological department of the University of Illinois.

DISCUSSION AND CORRESPONDENCE

PRACTICAL NOMENCLATURE

Dr. Needham's proposal[1] to use numbers in place of specific names in zoology fills me with astonishment. Granting that the problems of nomenclature are at bottom problems of psychology, what can be said in defense of a number-system as against one of names? Every man, woman and child in the world, with rare exceptions, I suppose, has a name. Every town or village has a name. Imagine that instead, we were all numbered, and that in order for this communication to reach the editor I had to write upon the envelope 21,560, A 493, X 2. Is that easier to remember than the customary address? Does it call up pleasanter thoughts? Garrison-on-Hudson, if it does consist of three words and sixteen letters, is pleasing and suggestive; were it twice as long I would not exchange it for a group of numbers. Even Tin Cup and Hell Gate, places in Colorado, have names which are suggestive and interesting, far better than, say 206 and 508. It is true that some names are unfortunate, but even the worst have a certain individuality, and with the authors indicated recall to us something of zoological history, often of romance.

Take the very list given by Dr. Needham. What must be the condition of a man's mind, if he thinks that numbers are a good exchange for *barbara, sponsa, nympha, forcipata, dryas* and the rest? What a fine century of entomological effort is called to our mind as we run over the names of Fabricius, Charpentier,

[1] Science, September 2, p. 295 et seq.

Fonscolombe, Say, Rambur, Hagen, de Selys and the others! All this might be thrown away, were there any compensating gain, but so far as I can see, there is only loss. It is not easier to remember numbers than names; on the contrary, they are much more readily forgotten, transposed or misprinted, and when mixed up they contain no clue to enable us to set them right.

I have worked many years at different branches of zoology and botany, and venture to affirm that it is easier to remember names than species. The names which come before us as a chaotic multitude, menacing and incomprehensible, *are those of things we do not know.* To me, even these names have a sort of charm, like that of unknown people passing in the street, each one a little mystery, with wonderful if unknown history and meaning. A high degree of complexity in nomenclature is reached when we attempt to indicate all sorts of minor categories, subgenera, subspecies and the like, but all this is for the purpose of reflecting in some poor way the real complexity of nature. The mind can not grasp it all, but it is possible to attain a reasonable comprehension of parts, and for this it seems to me that nomenclature (not numeration) is a useful tool. I am the more convinced that we are on the whole doing well, from the fact that in practically every group which I have studied, the path of the student is far easier to-day than it was twenty years ago.

T. D. A. COCKERELL
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SCIENTIFIC BOOKS

Studien ueber die Bestimmung des weiblichen Geschlechtes. Dr. ACHILLE RUSSO. Pp. iv + 105; 32 figures. Jena, Gustav Fischer. 1909.

In this brochure Professor Russo, of the Imperial University of Catania, has presented in German a compilation of the results that he has already announced in Italian publications, together with abstracts of more recent and unpublished work. The title of the present paper would indicate that its author has dealt only with the determination of the female sex, but as a matter of fact he outlines a series of experiments designed to show that sex is a question of maternal metabolism and that Mendelian dominance is similarly dependent upon conditions of nutrition in the mother. It is apparent, therefore, that the conclusions of Professor Russo upon the subjects of sex determination and Mendelian inheritance are widely at variance with those held by the majority of his fellow workers in these lines of investigation. Should he be found correct, much of the work of cytologists and experimental breeders of the last ten years is seriously in error. For this reason his data should be carefully considered in order to determine whether he is justified in opposing the prevailing opinions regarding the subjects he discusses.

The material is presented under three headings: I., General Part, wherein the author gives his conclusions and a summary of his results; II., Analytical Part, in which is considered the function of the epithelium of the rabbit ovary and the experimental proof to show that this is under control by artificial means; III., Experimental Part, where the results of the breeding trials are given and criticisms of the work of other investigators following his methods are presented. The line of reasoning pursued by Professor Russo is, in brief, this: Sex and the characters of the soma in the offspring, at least so far as pigmentation is concerned, are the result of the metabolism in the mother at the time the eggs are produced and made ready for fertilization. The maternal condition impresses itself upon the egg through the medium of the epithelium of the ovary. Preponderant anabolism results in the production of large proportions of females, while the opposite condition favors the production of males. Likewise favorable conditions of nutrition in the mother reverse the factors of dominance in Mendelian inheritance. So far as the matter of sex determination is concerned it is apparent that we have here a revival of the epigamous theory so thoroughly and ably presented by Geddes and Thompson. The modification of Mendelian characters is, however, something en-

tirely new and directly opposed to the results attained by all experimental breeders.

The method of controlling the maternal metabolism is through the injection of lecithin subcutaneously and intraperitoneally or even by feeding. According to Professor Russo the ovaries of such artificially nourished rabbits are very much larger than normal ones, and the mechanism of this transfer of the food material (lecithin) from the peritoneal cavity is traceable through the germinal epithelium into the ovary, past the stroma to the stratum granulosum of the Graafian follicles, through this and the cells of the corona radiata and finally into the ovum. He detects two general classes of eggs in the ovary; those with large deutoplasmic content and others having little or no food material. The former class is greatly increased by the use of lecithin, and such females as have been thus artificially nourished are said to produce females exclusively or in relatively large numbers. Also the author claims that the young of such mothers reproduce her characters of pigmentation even though they be recessive and in the presence of dominant characters introduced by breeding her with a dominant bearing male. In the last analysis therefore, according to Professor Russo, sex and somatic structure are determined by the nutrition of the mother acting through the medium of the ovary upon the eggs.

It is essential to Professor Russo's contention to prove that the ovary is really an organ of absorption and that it is capable of being influenced by the soma. To this part of his subject he devotes 68 out of the 105 pages of the paper. He presents the evidence from a study of normal ovaries in rabbits of various ages from two months to maturity and in different degrees of reproductive activity, and adds to this results derived from the study of artificially nourished ovaries. Gonads from starved females were also studied. The character of the germinal epithelium, of the stroma, of the stratum granulosum and liquor folliculi and of the zona pellucida under the various conditions of the experiments is described. A detailed consideration of the extra nuclear bodies and their chemical and morphological natures follows. The author's purpose in this second part is indicated in the following passages:

> Auf diesen, fuer die gegenwaertigen Untersuchungen fundamental wichtigen Punkt muss ich entschieden beharren, dann wenn auch einige in der Organismus eingefuehrte Stoffe sich als unwirksam, mitunter auch als schaedlich sich erwiesen, werden andere dagegen vom Eierstock aufgesogen und somit durch ihr Eindringen in denselben im deutoplasmatischen Material verwandelt. . . . Der Zweck dieses zweiten (II.) Teils findet weiter auch darin seinen Grund: den Beweis zu erbringen, dass der Eierstock der Saugetiere, im Gegensatz zur allgemein herrschenden Ansicht, nach welcher er, weil tief im Soma gelegen, den experimentellen Angriff gegenueber unverletzlich sei, in seiner inneresten Struktur durch verschiedenartig Mittel modifiziert kann.

In the final section of the work there is presented the results of a series of breedings between rabbits of different races intended to show that characters in new races, ordinarily recessive, may, under conditions of over nutrition, be made dominant. The experiments to show the effect of nutrition upon the proportions of the sexes, together with a consideration of the normal ratios of the sexes in rabbits, are also outlined here. Finally, the technical methods of administering the artificial food products and of preparing the material for study are given.

Comment.—It is perhaps a little unfair to judge an investigator's work by such a compilation of it as Professor Russo presents in this publication, but since he has prepared it specifically for the purpose of representing his exact attitude in respect to the subjects of sex determination and Mendelian inheritance it will be necessary to judge his work and opinions by this presentation of it. A general consideration of the paper gives one the impression that its author has not been thoroughly critical in his methods, and this feeling is intensified as details are studied. The Experimental Part, for instance, contains only about eight pages, largely illustrations, of experimental results, while the Analytical Part

is mainly occupied with the results of experiments. Further instances of this lack of discrimination will appear in the consideration of the various topics discussed. The bibliographies are extensive, but the references to them are comparatively few. The large amount of recent work upon sex determination by cytologists and experimental breeders receives but slight mention and, when referred to, is apparently not correctly understood. Professor Russo promises, however, an early consideration of this part of his subject and it is to be hoped that he will then make some effort to show the errors of those who find sex independent of external conditions. In commenting upon the details of Professor Russo's investigations it will be convenient to ask certain questions and to examine the evidence which he adduces in his replies to these.

First it may be asked if there are two recognizably distinct types of eggs in the rabbit ovary. An answer to this question can hardly be given justly from a mere inspection of the evidence in the paper. The figures presented are few and apparently indicate a morphological difference between the eggs, but there is no attempt made to determine the relative numbers of these under normal conditions or to show that there is a lessening of one type to accompany the increase of the other under the conditions of the experiment. It must also be recalled that experienced investigators like Heape[1] not only fail to find two types of normal eggs, but regard the supposedly male-producing eggs of Russo as those in process of degeneration. It can at least be said that the great importance attached to this part of his work by the author would require his determinations to be made more exact if they are to be effective as an argument in the minds of those familiar with ovarian histology.

Also it may be asked, if there are two normal types of eggs, whether it is possible to change one into the other by external influences. Russo's theory requires that this be done, but the evidence that he brings forward in support of his contention that this has been done is far from conclusive. Having artificially nourished female rabbits with lecithin, he kills them and studies the ovaries and reports that the proportion of fat containing eggs has been greatly raised. Similarly treated rabbits are bred and the proportion of females is said to be much increased. It is, therefore, concluded that one type of egg has been changed into the other. In addition to this evidence, which is all that there is to connect form variation of eggs with sexual characters, Russo presents the results of experiments upon fasting rabbits to show that the fat within the eggs disappears completely, and also those upon lecithin treated young to demonstrate that the fat is here brought into existence in eggs that normally do not acquire it until much later. None of this evidence proves that one kind of egg is changed into the other, but only that the food material may be increased or diminished by feeding and starving. So far as the histological part of the work is concerned, therefore, it may be said that the evidence brought forward seems to indicate that injections of lecithin may affect the metabolism of the ovary and its germ cells, but that there is no proof of the view that such treatment is effective in transforming one kind of egg into another.

The question may next be asked: Does the treatment of the maternal parent by the injection of lecithin alter the proportion of the sexes? Russo says very positively that it does and that the proportion of females may be raised from approximately 50 per cent. to a very much higher one, even to 100 per cent. in individual cases. Such results as these seem unequivocal enough, but the same experiments have been repeated by Basile[2] and by Punnett[3] and these investigators fail entirely to substantiate the claims made by Russo for his methods. It is pointed out by them and more recently by Castle[4] that Russo gave only selected results and failed

[1] Heape, W., *Proc. Cambridge Phil. Soc.*, Vol. 14, 1908, p. 609.

[2] Basile, C., *Atti Acad. Lincei*, Vol. 17, 1908.

[3] Punnett, R. C., *Proc. Cambridge Phil. Soc.*, Vol. 15, 1909.

[4] *The American Naturalist*, Vol. 44, No. 523, 1910.

to present the whole series of experiments from which he drew his conclusions, while in the repeated experiments of Basile and Punnett all results were tabulated. This unscientific attitude seems to pervade the whole of Russo's work, and so long as his methods are thus at fault, it is not worth while to consider the bearing of his results, particularly in the face of direct contradiction by other investigators going over the same ground.

Finally the enquiry concerning the possibility of reversing the operation of Mendelian dominance in cross breedings may be considered. Here again we have to do with faulty experimental methods. Russo claims to be able to make white dominant over black in the first generation of hybrids by treating the white mother with lecithin injections before breeding, but practically no attempt is made to analyze the racial composition of the animals used in breeding. Such experiments as he presents in support of this contention would have no standing whatever with experienced breeders and it may be said without any exaggeration that in such a presentation of his case he has forfeited entirely the serious consideration of his work. A detailed analysis of this part of his studies has been recently given by Castle[8] and will not be repeated here. It is much to be regretted that an extended investigation like this of Russo's should be vitiated by untrustworthy methods, for such lines of work need following out and are extremely valuable in furthering an analysis of the relations existing between the germ cells and the parental bodies. That the author will present his work purged of the serious errors it now contains must be the hope of all his fellow workers.

C. E. McClung

Factor Tables for the First Ten Millions, containing the smallest factor of every number not divisible by 2, 3, 5 or 7 between the limits 0 and 10,017,000. By Derrick Norman Lehmer. Washington, D. C., Carnegie Institution of Washington, Publication No. 105. 1909. Pp. xvi + 476. Price $20.

The publication of the best and most extensive work in any language, on an old and important subject, is eminently worthy of recognition, especially when the preparation of such a work demanded the most painstaking care and unselfish devotion to the interests of science. Prime numbers and factors of composite numbers are among the oldest as well as among the newest objects of study in mathematics. The perennial interest in these subjects bears testimony to their importance and helpfulness in our efforts towards stronger instruments of thought and towards a more rational intellectual penetration into the physical laws which we encounter on all sides.

While it may be true that integral numbers do not occupy comparatively as large a place in our present mathematical thinking as they once did, they still constitute, according to Minkowski, "the fountain-head of all mathematics" and they enter prominently into many of our mental processes. We are not infrequently brought to questions whose solutions are expedited by a knowledge of the existence of primes or of the factors of large composite numbers. Under such circumstances one will naturally turn hereafter to the tables before us with an unusual confidence in their correctness and a high appreciation of their great extent.

The pages of the present table are very large—about sixteen inches long and twelve inches wide. "Each horizontal line of the table covers 210 numbers. The multiples of 2, 3, 5 and 7 are not listed. As there are 100 lines on each page it follows that each page will serve to find the smallest factor of 21,000 consecutive numbers. The largest and smallest of these are given at the top of the page. These numbers then indicate at a glance the page that contains the smallest divisor of the given number." To find the smallest factor of a given number without the aid of an auxiliary table, it is necessary to divide the number by 210 and to locate the quotient and the remainder in the table. By means of these two numbers it is very easy to find the smallest factor of the number in question, if it is composite but not divisible by 2, 3, 5 or 7. The division by 210 may be avoided by means of an auxiliary table.

In his preface the author states that "The

[8] *Loc. cit.*

Carnegie Institution of Washington has for five years furnished the funds necessary for the preparation of the manuscript and for the publishing of the tables." He also acknowledges gratefully sufficient temporary relief from academic work in the University of California to afford opportunity to devote more of his time to the arduous task of most careful proof-reading, for errors in such work are not suggested by the context, and the author wisely observes that "the value of a factor table depends chiefly on its freedom from errors."

The introduction includes a valuable list of corrections to earlier extensive tables and directs attention to "the manuscripts of Kulik which were placed in charge of the Vienna Royal Academy in 1867. These tables were said to give the smallest factor of all numbers not divisible by 2, 3 or 5 up to the limit of *one hundred million!*" The author of the present table saw only the first one of the six volumes of Kulik's manuscript, and furnishes a rather extensive list of errata in the tenth million. He also includes, in the introduction, a short historical account of the earlier factor tables as well as some remarks on the methods of constructing such tables. In every way the present table appears to deserve a very high place among the American mathematical publications of permanent value, and both the author and the Carnegie Institution have rendered a great service not only to the mathematical public but also to many who make only occasional use of mathematics.

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SHACKLETON'S CONTRIBUTION TO BIOGEOGRAPHY

IT has long been surmised that certain south polar lands may have one time connected several of the main biogeographic regions of the earth. Wilkes Land, South Victoria Land and Graham Land, with other near-by lands more recently named, have been conceived as forming a continent, which in times past may have stretched its shores to connections with the other continents of the southern hemisphere. Shackleton's recent work in Antarctica has now placed the existence of that continent in the realm of fact. Surmise has given way to certainty. We are now in position to deduce certain conclusions from its existence and the known conditions pertaining to it. In the hope of stimulating discussion of the general subject by those more versed in paleogeographic data than myself, I venture to state the following aspects of Shackleton's discoveries as they appear to the student of geographical distribution.

During past geologic ages, with the exception of certain relatively brief intervals of change, Antarctica has, in common with the rest of the globe, enjoyed comparative freedom from ice, excepting only the presence of alpine glaciers, and been blessed with an equable temperature. In those days the wide-stretching south polar land comprised an immense continent whose thousands of miles of extent were for the most part quickened by a mild climate and populated with an abundant life. Here during Paleozoic, Mesozoic and Tertiary time was a wonderfully rich territory, its resources now practically lost to us under an all-pervading ice-sheet. Shackleton's party found evidence of extensive coal deposits, including remains of forested areas, indicating an abundant flora and fauna. Let us see what light the former existence of such favorable biotic conditions throws on the present distribution of life with reference to Australia, South America and Africa.

Australia and New Zealand, occupying approximately longitude 110° to 180° east from Greenwich, are almost opposite the southern extremity of South America, which is about longitude 70° west from Greenwich. The southern limits of Australia and New Zealand are in latitude 40° to 50° south, those of South America in latitude 55° south. Thus there intervenes between these present land divisions an actual distance of only 75° to 85° by way of the south pole. The straight-away line between the centers of the two masses passes well to one side of the pole, and the intervening distance between their southern limits, but practically across the heart of the south polar region, may be roughly stated as 4,500 geographic miles. The southern ocean soundings so far made reveal shallow depths, or epi-

continental seas, between the Australian-New Zealand region and Antarctica (Wilkes Land and South Victoria Land, the two doubtless continuous) on the one hand, and South America and Antarctica (Graham Land) on the other. We are quite justified in believing that these epicontinental sea-bottoms were land areas for long periods of time and have since subsided beneath the ocean level.

Here then was the land connection which furnished not only the highway for the interchange of forms of life between the southern continents, but also a vast territory of subpermanent residence and consequent multiple development of those forms during the intervals that elapsed between their successive wanderings. So far as area goes, it was practically as though the continent of Africa were to be laid down on the south polar region to-day, with its center on the pole. But it would be an Africa well watered throughout, even somewhat increased in size, perhaps not much changed in contour, with very different faunal and floral elements.

Alexander Land is probably a continuation of Graham Land, and King Edward VII Land is an extensive reach that probably connects these with South Victoria Land. On the opposite side, Coats Land and Enderby Land probably meet and form a land reach directly south of Africa, probably also continuing to Graham Land on the one side and to Wilkes Land on the other. Thus we almost certainly have to-day in Antarctica one vast compact land mass forming a continent more than twice the size of Australia, or even larger than South America and almost the counterpart of the latter in outline. There is no such continental mass in the north polar region.

In former ages the elevation of Antarctica above sea level was, as a matter of course, much less than now. Shackleton has found to exist there at present an immense plateau-continent with twice the average altitude of Asia—the highest of the other continents. The upheaval of Antarctica has certainly been going on for ages. During Tertiary times the continent was probably not dissimilar in average elevation to present-day South America and Asia, and its subsequent further uplifting during periods of diastrophic activity has been the immediate cause of its present isolation through the subsidence of its shelf-lines. Probably the Australian-Antarctic-South American land-connection was maintained from early Paleozoic time to somewhere about the Jurassic period. This is indicated by the present distribution of the muscoid flies in Australia and South America. The writer would like to know whether this view is borne out by a study of other elements of the two faunæ. At all events, it appears that South America was the first to lose its connection wih the southern continental mass.

Africa was also quite certainly connected with Antarctica, and through it with South America and Australia, its connection with the main mass having apparently persisted to a much later date than that of South America, if we may credit the evidence of present muscoid fly distribution in the continents concerned. Comparatively shallow depths must exist somewhere between the African region and Antarctica. The southern extremity of Africa is in latitude 35° south, and its connection with the southern continental mass implies a land stretch of some 5,000 geographic miles in opposite directions to Australia and South America respectively, along the edges of the south polar region. It is likely that the African connection was maintained by a comparatively narrow isthmus, and until about Miocene times.

Add to Antarctica as above restored the great extents of land area represented by the Paleozoic and Mesozoic continents of South America, Africa and Australia, all continuous, with many high mountain ridges interspersed, all in the main under a mild and equable climate, and we have a vast range of land surface which, in its possibilities for the evolution of varied forms of life, quite staggers the imagination. It is even possible to imagine a land connection of Africa and South America, on the one hand, through Antarctica and Australia, with, on the other hand, Malaysia and the continent of Asia. There has certainly been connection of Malaysia with Australia, and probably with the Asiatic mainland whose original confines were the general

Himalayan region. The possibilities of the biogeographic vista of remote antiquity opened up to us by the existence of Antarctica are enormous, and quite equal to the task of explaining the many hitherto perplexing problems of bio-distribution present and past.

It remains to establish the existence of the former contacts of Antarctica with the southern continents during times past, and the duration of those several contacts until the last one was severed and the present complete isolation of the continent effected. Such contacts are indicated by a study of the faunæ of to-day. Their former existence may be established by the determination of epicontinental seas, continental platforms and submerged ridges, in and about the regions in question. Their duration may be revealed by a study of the geological history of organisms coupled with that of present-day biogeography. We may look forward with the liveliest interest to the much-to-be-desired paleontologic results which should be forthcoming from the further south polar expeditions now outfitting. Certainly here is the field for fruitful investigation of the phylogeny of late and early forms of life, can one but withstand the rigors of its present climate.

It would now appear that the question of the one-time existence of the fabled continent Gondwana, furnishing an east-and-west connection between South America, Africa and Australia, may be relegated to oblivion; the more decidedly so in view of the quite certainly established permanence throughout geologic time of the present ocean basins. Antarctica is doubtless the real Gondwana, but in another quarter—the southern!

CHARLES H. T. TOWNSEND

PIURA, PERU,
July 4, 1910

THE INFLUENCE OF NUTRITION UPON THE ANIMAL FORM

THE above-named paper by H. J. Waters, presented at the thirtieth meeting of the Society for the Promotion of Agricultural Science, is reviewed because it appeared in an agricultural publication and may not otherwise come to the attention of the experimental morphologist and others to whom it may be of considerable interest. Mr. Waters reports some experiments that were made at the Agricultural College of the University of Missouri. A number of young beef steers were kept during the growth period on different planes of nutrition. One group were fed so as to allow a gradual increase in weight (supramaintenance); a second group were so fed that they kept a constant weight (maintenance); a third group were fed so that they gradually lost weight (submaintenance). The animals were measured carefully at regular intervals during the experiment. The results show that even in the submaintenance animals the skeleton continues to grow for a long time, but its growth is retarded and after several months checked completely. The point of greatest interest is the disproportionate growth of the skeleton in the underfed animals. The ratio of the total increase in the width of the hips to the total increase in height at the withers during the entire experiment is approximately as follows: in the supramaintenance group, 1:2; in the maintenance group, 1:3, in the submaintenance group, 1:5. Underfeeding retards the increase in the width of the skeleton at the hips much more than it retards its increase in height. In other words the skeleton of a beef steer grows much wider in proportion to its height when the animal is well fed than when it is poorly fed. The author is inclined to attribute the expansion of the skeleton typically seen in beef cattle to the continuous pressure of the distended alimentary canal.

It is interesting to note that the ancestral type, from which the modern beef animal has been derived, corresponds in the shape of the skeleton to the underfed animals described above. Stockmen have insisted for many years that the best bred beef animals, when kept under range conditions, will assume in a few generations what is commonly known as the "sun-fish" type, or an approximation to the ancestral type. The narrowing of the skeleton in response to an inadequate food supply may be a physiological adaptation, or it may be a case of reversion. E. T. BELL

SPECIAL ARTICLES

THE ISOLATION OF AN ION, A PRECISION MEASUREMENT OF ITS CHARGE, AND THE CORRECTION OF STOKES'S LAW[1]

§ 1. *Introduction.*—There is presented herewith a new method of studying gaseous ionization, with the aid of which it has been found possible:

1. To catch upon a minute droplet of oil and to hold under observation for an indefinite length of time one single atmospheric ion or any desired number of such ions between 1 and 150.

2. To present direct and tangible demonstration, through the study of the behavior in electrical and gravitational fields of this oil drop carrying its captured ions, of the correctness of the view advanced many years ago and supported by evidence from many sources that all electrical charges, however produced, are exact multiples of one definite, elementary, electrical charge; in other words, that an electrical charge, instead of being spread uniformly over a charged surface, has a definite granular structure, consisting, in fact, of an exact number of specks, or atoms of electricity, all precisely alike, peppered over the surface of the charged body.

3. To make an exact determination of the value of this elementary electrical charge, which is free from all questionable theoretical assumptions and is limited in accuracy only by the accuracy which is attainable in the measurement of the coefficient of viscosity of air.

4. To observe directly the order of magnitude of the kinetic energy of agitation of a molecule, and thus to bring forward new, direct and most convincing evidence of the correctness of the kinetic theory of matter.

5. To demonstrate that the great majority of the ions of the air of both positive and negative sign, carry the elementary electrical charge, and to present convincing evidence that some atmospheric ions carry exact multiples of this charge; in other words, that the phenomena of valency are exhibited to some extent in gaseous ionization.

6. To show that the law of motion of a small sphere through a resisting medium, commonly known as Stokes's law, breaks down as the diameter of the sphere becomes comparable with the mean free path of the molecules of the medium, and to determine the exact way in which it breaks down.

The investigation by means of which these results have been obtained differs from most of the equally important ones which are carried on in the physical laboratory, in that the method used is so simple, and the conclusions follow so inevitably from the experimental data that even the man on the streets can scarcely fail to understand the method or to appreciate the results.

§ 2. *The Method.*—The method by which these results have been obtained and by which still further important results bid fair to be obtained grew out of some experiments which were presented in a preceding paper.[2] It is in brief as follows: A cloud of fine droplets of oil, or of mercury, or of some other non-volatile substance is blown by means of an atomizer[3] over a horizontal air condenser and a few of the droplets in this cloud are allowed to fall through a pinhole in the middle of the upper plate of this condenser into the space between the plates. The pinhole is then closed for the sake of shutting out air currents. The condenser used consists in most of the experiments of two heavy, circular, and accurately planed brass plates, 20 cm. in diameter, held exactly 16 mm. apart by means of three small ebonite posts. The plates are

[1] At the request of the editor this abridgment of a paper presented on April 23, 1910, before the American Physical Society is published in SCIENCE.

[2] *Phil. Mag.*, 19, p. 209, 1910.

[3] The atomizer method of producing very minute but accurately spherical drops for the purpose of studying their behavior in fluid media, was first conceived and successfully carried out in January, 1908, at the Ryerson Laboratory, by Mr. J. Y. Lee, while he was engaged in a quantitative investigation of Brownian movements. His spheres were blown from Wood's metal, wax and other like substances which solidify at ordinary temperatures. Since then the method has been almost continuously in use here, upon this and a number of other problems, and elsewhere upon similar problems.

enclosed, and the temperature controlled so that the air within the condenser is altogether stagnant. The droplet, once inside the condenser, is illuminated through a small window by a beam from an arc light, so that it appears in the field of view of the observing cathetometer telescope like a bright star on a black background. This star, of course, falls under the action of gravity toward the lower plate, but before it reaches it, an electrical field of strength between 3,000 volts and 8,000 volts per centimeter is thrown on between the plates, and, if the droplet had received a charge of the proper sign and strength as it was blown out through the atomizer, it is pulled up by this field against gravity, toward the upper plate. Before it strikes this plate the field is thrown off, the plates short-circuited, and the time required by the drop to fall under gravity the distance corresponding to the space between the cross hairs of the observing telescope is accurately determined. Then the rate at which the droplet moves up under the influence of the field is measured by timing it through the same distance when the field is on. This operation is repeated and the speeds checked an indefinite number of times, or until the droplet catches an ion from among those which exist normally in air, or which have been produced in the space between the plates by any of the usual ionizing agents like radium or X-rays. The fact that an ion has been caught, and the exact instant at which the event happened are signaled to the observer by the change in the speed of the droplet under the influence of the field. From the sign and magnitude of this change in speed, taken in connection with the constant speed under gravity, the sign and the exact value of the charge carried by the captured ion are determined. The error in a single observation need not exceed one third of one per cent. Furthermore, it is from the values of the speeds observed that all of the conclusions above mentioned are directly and simply deduced.

§3. *The Deduction of the Relative Values of the Charges Carried by a Given Droplet.*—The relations between the mass m of a drop, the charge e_n, which it carries, its speed v_1 under gravity, and its speed v_2, under the influence of an electrical field of strength F, are given by the simple equation

$$\frac{v_1}{v_2}=\frac{mg}{Fe_n-mg} \quad \text{or} \quad e_n=\frac{mg}{F}(v_1+v_2). \qquad (1)$$

This equation involves no assumption whatever save that the speed of the drop is proportional to the force acting upon it, an assumption which is fully and accurately tested experimentally in the following work. Furthermore, equation (1) is sufficient not only for the correct determination of the relative values of all of the charges which a given drop may have after the capture of a larger or smaller number of ions, but it is also sufficient for the establishment of all of the assertions made above, except 3, 4 and 6, and for the establishment of 4 no other exact relationship is needed. However, for the sake of obtaining a provisional estimate of the value of m in equation (1), and therefore of making a provisional determination of the absolute values of the charges carried by the drop, Stokes's law will, for the present, be assumed to be correct, but it is to be distinctly borne in mind that the conclusions just now under consideration are not at all dependent upon the validity of this assumption.

This law states that if μ is the coefficient of viscosity of a medium, X the force acting upon a spherical drop of radius a in that medium, and v the velocity with which the drop moves under the influence of the force, then

$$X=6\pi\mu av. \qquad (2)$$

The substitution in this equation of the resulting gravitational force acting on a spherical drop of density σ in a medium of density ρ gives the usual expression for the rate of fall, according to Stokes, of a drop under gravity, viz.,

$$v_1=\frac{2}{9}\frac{ga^2}{\mu}(\sigma-\rho). \qquad (3)$$

The elimination of m from (1) by means of (3), and the further relation

$$m = \tfrac{4}{3}\pi a^3 \alpha$$

gives the charge e_n in the form

$$e_n = \tfrac{4}{3}\pi \left(\frac{9\mu}{2g(\sigma - \rho)}\right)^{\frac{3}{2}} \frac{\sigma g}{F} (v_1 + v_2) v_1^{\frac{1}{2}}. \qquad (4)$$

It is from this equation that the values of e_n in tables I.–XI. are obtained.

§ 4. *Preliminary Observations upon the Catching of Ions by Oil-drops.*—Table I. presents the record of the observations taken upon a drop which was watched through a period of four and one half hours as it was alternately moved up and down between the cross-hairs of the observing telescope under the influence of the field F and gravity G. How completely the error arising from evaporation, convection currents, or any sort of disturbances in the air, are eliminated, is shown by the constancy during all this time in the value of the velocity under gravity. This constancy was not attained without a considerable amount of experimenting which will be described in full elsewhere. It is sufficient here to state that the heating effects of the illuminating arc were eliminated, first by filtering the light through about two feet of water, and second, by shutting off the light from the arc altogether except at occasional instants, when the shutter was opened to see that the star was in place, or to make an observation of the instant of its transit across a cross-hair. Further evidence of the complete stagnancy of the air is furnished by the fact that for an hour or more at a time the drop would not drift more than two or three millimeters to one side or the other of the point at which it entered the field.

The observations in Table I. are far less accurate than many of those which follow, the timing being done in the case of Table I. with a stop-watch, while many of the later timings were taken with a chronograph. Nevertheless, this series is presented because of the unusual length of time over which the drop was observed, and because of the rather unusual variety of phenomena which it presents.

The column headed G shows the successive times, in seconds, taken by the droplet to fall under gravity the distance between the cross-hairs. It will be seen that in the course of the four and one half hours the value of the time increases very slightly, thereby showing that the drop is very slowly evaporating. Furthermore, there are rather marked fluctuations recorded in the first ten observations which are probably due to the fact that, in this part of the observation, the shutter was open so much as to produce very slight convection currents.

The column headed F is the time of ascent of the drop between the cross hairs under the action of the field. The column headed e_n is the value of the charge carried by the drop as computed from equation (4). The column headed n gives the number by which the values of the preceding column must be divided to obtain the numbers in the last column. The numbers in the e_n column are in general averages of all the observations of the table which are designated by the same numeral in the n column. If a given observation is not included in the average in the e_n column, a blank appears opposite that observation in the last two columns. On account of the slow change in the value of G, the observations are arranged in groups and the average value of G for each group is placed opposite that group in the first column. The reading of the PD between the plates, taken at the mean time corresponding to each group, is labeled V and placed just below or just above the mean G corresponding to that group. The PD was applied by means of a storage battery.

§ 5. *Discussion of Table I.*—Since the original drop was in this case negative, it is evident that a sudden increase in the speed due to the field, that is, a decrease in the time given in column F, means that the drop has caught a negative ion from the air, while a decrease in the speed means that it has caught a positive ion. If attention be directed, first, to the latter part of the table, where the observations are most accurate, it will be seen that beginning with the group for which $G =$

TABLE I

Negative drop

Distance between cross hairs = 1.010 cm.
Distance between plates = 1.600 "
Temperature = 24.6° C.
Density of oil at 25° C. = .8960
Viscosity of air at 25.2° C. = .0001837

	G (sec.)	F (sec.).	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
	22.8	29.0	7	34.47	4.923
	22.0	21.8	8	39.45	4.931
	22.3	17.2			
$G = 22.28$	22.4	—	9	44.42	4.936
$V = 7950$	22.0	17.3			
	22.0	17.3			
	22.0	14.2	10	49.41	4.941
	22.7	21.5	8	39.45	
	22.9	11.0	12	59.12	4.927
	22.4	17.4	9	44.42	
	22.8	14.3	10	49.41	
$V = 7920$	22.8	12.2	11	53.92	4.902
$G = 22.80$	22.8	12.3			
	23.0	—			
	22.8	14.2			
$F = 14.17$	—	—	10	49.41	4.941
	22.8	14.0			
	22.8	17.0			
$F = 17.13$	—	17.2	9	44.42	4.936
	22.9	17.2			
	22.8	10.9			
$F = 10.73$	22.8	10.9	12	59.12	4.927
	22.8	10.6			
	22.8	12.2	11	53.92	4.902
$V = 7900$	22.8	8.7	14	68.65	4.904
$G = 22.82$	22.7	6.8	17	83.22	4.894
$F = 6.7$	22.9	6.6			
	22.8	7.2			
	—	7.2			
	—	7.3			
$F = 7.25$	—	7.2	16	78.34	4.897
	23.0	7.4			
	—	7.3			
	—	7.2			
$F = 8.65$	22.8	8.6	14	68.65	4.904
	23.1	8.7			
	23.2	9.8	13	63.68	4.900
	—	9.8			
$F = 10.63$	23.5	10.7	12	59.12	4.927
	23.4	10.6			
	23.2	9.6			
	23.0	9.6			
	23.0	9.6			
	23.2	9.5			
$V = 7820$	23.0	9.6	13	63.68	4.900
$G = 23.14$	—	9.4			
$F = 9.57$	22.9	9.6			
	—	9.6			
	22.9	9.6			
	—	10.6	12	59.12	4.927
$F = 8.65$	—	8.7	14	68.65	4.904
	23.4	8.6			
	23.0	12.3			
$F = 12.25$	23.3	12.2	11	53.92	4.902
	—	12.1			
	23.2	12.4			
	Change forced with radium.				

	G (sec.)	F (sec.)	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
	23.4	72.4			
	22.9	72.4			
$F = 72.10$	23.2	72.2	5	24.60	4.920
	23.5	71.8			
	23.0	71.7			
	23.0	39.2	6		
$V = 7800$	23.2	39.2			
$G = 23.22$	—	27.4	7	34.47	
	—	20.7	8	39.38	4.922
	—	26.9	7	34.47	4.923
	—	27.2			
	23.3	39.5			
	23.3	39.2	6	29.62	4.937
$F = 39.20$	23.4	39.0			
	23.3	39.1			
	23.2	71.8	5	24.60	4.920
	23.4	382.5	4		
	23.2	374.0			
	23.4	71.0	5	24.60	4.920
	23.8	70.6			
$V = 7760$	23.4	38.5	6		
$G = 23.43$	23.1	39.2			
	23.5	70.3			
	23.4	70.5			
	23.6	71.2	5	24.60	4.920
	23.4	71.4			
	23.6	71.0			
	23.4	71.4			
	23.5	380.6			
	23.4	384.6			
	23.2	380.0			
$F = 379.6$	23.4	375.4	4	19.66	4.915
	23.6	380.4			
	23.3	374.0			
	23.4	383.6			
	—	39.2			
$F = 39.18$	23.5	39.2	6	29.62	4.937
$V = 7730$	23.5	39.0			
$G = 23.46$	23.4	39.6			
	—	70.8			
$F = 70.65$	—	70.4	5	24.60	4.920
	—	70.6			
	23.6	378.0	4	19.66	4.915
	Saw it, here, at end of 305 sec., pick up two negatives				
	23.6	39.4	6	29.62	4.937
	23.6	70.8	5	24.60	4.920
				Mean of all $e_1 s$ =	**4.917**

Differences

24.60 — 19.66 = 4.94
29.62 — 24.60 = 5.02
34.47 — 29.62 = 4.85
39.38 — 34.47 = 4.91
Mean dif. = 4.93

23.43, the time of the drop in the field changed suddenly from 71 seconds to 380 seconds, then back to 71, then down to 39, then up again to 71, and then up again to 380. These numbers show conclusively that the positive ion

caught in the first change, *i. e.*, from 71 to 380, carried exactly the same charge as the negative ion caught in the change from 380 to 71; or again, that the negative ion caught in the change from 71 to 39, had exactly the same charge as the positive ion caught in the change from 39 to 71.

Furthermore, the exact value of the charge caught in each of the above cases is obtained in terms of mg from the differences in the values of e_n, given by equation (1), and if it be assumed that the value of m is approximately known through Stokes's law, then the approximately correct value of the charge on the captured ion is given by the difference between the values of e_n obtained through equation (4). The mean value of this difference obtained from all the changes in the latter half of table I. (see Differences) is 4.93×10^{-10}.

Now it will be seen from the first observation given in the table that the charge which was originally upon this drop and which was obtained not from the ions in the air, but from the frictional process involved in blowing the spray, was 34.47×10^{-10}. This number comes within one seventh of one per cent. of being exactly seven times the charge on the positive or on the negative ion caught in the observations under consideration. Mr. Harvey Fletcher and myself, who have worked together on these experiments since December, 1909, studied in this way between December and May from one to two hundred drops which had initial charges varying between the limits 1 and 150, and which were upon as diverse substances as oil, mercury and glycerine, and found in every case *the original charge on the drop an exact multiple of the smallest charge which we found that the drop caught from the air.* The total number of changes which we have observed would be between one and two thousand, and *in not one single instance has there been any change which did not represent the advent upon the drop of one definite invariable quantity of electricity, or a very small exact multiple of that quantity.* These observations are the justification for assertions 1 and 2 of the introduction.

Before discussing assertion 4 it is desirable to direct attention to three additional conclusions which can be drawn from table I.:

First, since the time of the drop in the field varied in these observations from 380 seconds to 6.7 seconds, it will be seen that the resultant moving force acting upon the drop was varied in the ratio 1 to 55, without bringing to light the slightest indication of a dependence of e_1 upon the velocity. Independently of theory, therefore, we can assert that the velocity of this drop was strictly proportional to the moving force. The certainty with which this conclusion can be drawn may be seen from a consideration of the following numerical data. Although we had upon our drop all possible multiples of the unit 4.917×10^{-10} between 4 and 17, save only 15, there is not a single value of e_1 given in the table which differs by as much as .5 per cent. from the final mean e_1. It is true that the observational error in a few of the smaller times is as much as 1 or 2 per cent., but the observational error in the last half of the table should nowhere exceed .5 per cent. In no case is there here found a divergence from the final value of e_1 of more than .4 per cent.

Second, since the charge on the drop was multiplied more than four times without changing at all the value of G, or the value of e_1, the observations prove conclusively that in the case of drops like this, the drag which the air exerts upon the drop is independent of whether the drop is charged or uncharged. In other words, the apparent viscosity of the air is not affected by the charge in the case of drops of the sort used in these experiments.

Third, it will be seen from the table that in general a drop catches an ion only when the field is off. Were this not the case there would be many erratic readings in the column under F, while in all of the four and one half hours during which these experiments lasted, there is but one such. A moment's consideration will show why this is. When the field is on, the ions are driven with enor-

mous speed to the plates as soon as they are formed, their velocities in the fields here used being not less than 10,000 cm. per sec. Hence an ion can not be caught when the field is on unless the molecule which is broken up into ions happens to be on the line of force running from the plates through the drop. With minute drops and relatively small ionization this condition is very unlikely to occur. When the field is off, however, the ions are retained in the space between the plates and sooner or later, one or more of them, by virtue of its energy of agitation, makes impact upon the drop and sticks to it.

These considerations lead up to assertion 4 in the introduction. It will be seen from the readings in the first half of the table that even when the drop had a negative charge of from 12 to 17 units it was not only able to catch more negative ions, but it apparently had an even larger tendency to catch the negatives than the positives. Whence then does a negative ion obtain an amount of energy which enables it to push itself up against the existing electrostatic repulsion and to attach itself to a drop already strongly negatively charged? It can not obtain it from the field, since the phenomenon occurs when the field is not on. It can not obtain it from any explosive process which frees the ion from the molecule at the instant of ionization, since again in this case, too, ions would be caught as well, or nearly as well, when the field is on as when it is off. *Here then is an absolutely direct proof that the ion must be endowed with a kinetic energy of agitation, which is sufficient to push it up to the surface of the drop against the electrostatic repulsion of the charge already on the drop.*

This energy may easily be computed as follows: As will appear later the radius of the drop was in this case .000197 cm.; furthermore, the value of the elementary electrical charge obtained as a mean of all of our observations, is 4.902×10^{-10}. Hence, the energy required to drive an ion carrying a unit charge up to the surface of a charged sphere of radius r, carrying sixteen elementary charges, is

$$\frac{16e^2}{r} = \frac{16 \times (4.901 \times 10^{-10})^2}{.000197} = 1.95 \times 10^{-14} \text{ ergs.}$$

Now the kinetic energy of agitation of a molecule as deduced from the value of e herewith obtained, and the kinetic theory equation, $p = \frac{1}{3}mnu^2$, is 5.756×10^{-14} ergs. According to the Maxwell-Boltzmann law, which doubtless holds in gases, this should also be the kinetic energy of agitation of an ion. It will be seen that the value of this energy is approximately three times that required to push a single ion up to the surface of the drop in question. If, then, it were possible to load up a drop with negative electricity until the potential energy of its charge were about three times as great as that computed above for this drop, then the phenomenon here observed, of the catching of new negative ions by such a negatively charged drop, should not take place, save in the exceptional case in which an ion might acquire an energy of agitation considerably larger than the mean value. Now, as a matter of fact, it was regularly observed that the heavily charged drops had a very much smaller tendency to pick up new negative ions than the more lightly charged drops. And in one instance Mr. Fletcher and myself watched for four hours a negatively charged drop of radius .000658 cm., which carried charges varying from 126 to 150 elementary units, and which therefore had a potential energy of charge (computed as above on the assumption of uniform distribution) varying from 4.6×10^{-14} to 5.47×10^{-14} ergs, and *in all that time this drop picked up but one single negative ion, and that despite the fact that the ionization was several times more intense than in the case of the drop in table I.* This is direct proof, independent of all theory, that the order of magnitude of the kinetic energy of agitation of a molecule is 5×10^{-14}, as the kinetic theory demands.

The first portion of assertion 5 is directly proven by the readings contained in the table, since the great majority of the changes recorded in column 4 corresponds to the addition or subtraction of one single elementary charge. The second portion of the assertion seems at first sight to be proven by the remaining

changes which correspond to the addition or subtraction of 2 or 3 times this amount. The conclusion, however, that valency is exhibited in gaseous ionization is not to be so easily drawn. The arguments for it which are furnished by our experiments will be presented fully elsewhere. Space here only permits the statement that the only strong argument furnished by table I. is found near the end of the table where, *when the field was on, the drop caught a double negative ion, while I was looking at it.*

Some idea of the intensity of ionization used in these experiments may be gained from the statement that during the observations recorded in the first half of the table, a closed tube of radium, containing 500 mg. of radium bromide of activity 3,000, stood about five feet away from the testing chamber, so that its γ rays could enter this chamber. At the end of the observations in the group in which $G = 23.14$, this radium was brought up to within a few inches of the testing chamber, and six elementary charges were forced upon the drop in a manner which will presently be explained. The radium was then taken entirely out of the room, so that the changes recorded in the last half of the table are entirely due to such ionization as exists in air under normal atmospheric conditions.

There is but one more comment to be made upon table I. At a point indicated in the table by the remark "change forced with radium," it will be noticed that the charge was suddenly changed from eleven negative units to five negative units, *i. e.*, that six positive units were forced upon the drop. This sort of a change was one which, after the phenomenon had once been got under control, we could make at will in either direction; *i. e.*, we could force charges of either sign or in any desired number, within limits, upon a given drop. We did this as follows: when it was desired to load the drop up negatively, for example, we held it with the aid of the field fairly close to the positive plate, and placed the radium so that it would produce uniform ionization throughout the chamber. Under these conditions, if the positive and negative ions were alike in number and mobility, the chance that the drop would catch a negative ion would be as many times its chance of catching a positive ion as the distance from the drop to the negative plate was times the distance from the drop to the positive plate. Similarly, if we wished to load the drop positively it was held by the field close to the negative plate. On account of the slightly greater mobility of the negative ions and also on account of the somewhat greater numbers in which they occur, we found, in general, a slightly greater tendency of the drops to take up negative than positive charges. In view, therefore, of the greater ease with which negative drops could be held for long intervals without being lost to the plates most of the drops studied have been of negative sign.

§ 5. *The Failure of Stokes's Law.*—When the values of e_1 were computed, as above, for different drops, although each individual drop showed the same sort of consistency which was exhibited by the drop of table I., the values of e_1 at first came out differently even for drops showing the same value of the velocity under gravity. This last irregularity was practically completely eliminated by blowing the drops into air which was strictly dust-free, but even then drops of different sizes as determined by v_1 always gave consistently different values of

TABLE II

Negative drop No. 5

Distance between cross hairs = 1.303 cm.
Temperature = 24.6° C.
Density of oil at 25° C. = .9041

	G (sec.)	F (sec.)	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
	120.8	26.2	2	10.98	5.490
$F = 11.9$	121.0	11.9	4	21.98	5.495
	121.2	16.5	3	16.41	5.470
	120.1	16.3			
$F = 26.40$	120.2	26.4	2		
	119.8	67.4	1	5.495	5.495
$G = \mathbf{120.07}$	120.1	26.6	2	10.98	
$V = 9150$	—	16.6			
	120.2	16.6	3	16.41	
$F = 16.50$	—	16.5			
$F = 67.73$	120.2	68.0	1	5.495	
	119.9	67.8			
		26.4		10.98	

$v_1 = \mathbf{.01085}$ Mean e_1 (weighted) $= \mathbf{5.490}$

TABLE III

Negative drop No. 8

Distance between cross hairs = 1.033 cm.
Temperature = 20° C.

	G (sec.)	F (sec.)	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
	88.0	—			
$V = 3512$	88.8	95.3	2	10.98	5.490
$G = $ **87.85**	87.8	31.0	4	21.93	5.482
	87.4	30.8			
$F = 30.9$	87.8	47.0	3	16.41	5.470
	87.3	—			

v_1 — **.01176** Mean e_1 (weighted) = **5.482**

TABLE IV

Negative drop No. 12

Distance between cross hairs = 1.005 cm.
Temperature = 24.3° C.

	G (sec.)	F (sec.)	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
$F = 49.15$	53.8	49.2	4	21.46	5.365
	53.7	49.1			
$G = $ **53.80**	54.0	95.2			
$V = 3990$	—	95.5	3	16.00	5.333
$F = 95.78$	53.7	96.6			
	53.7	95.8			

v_1 = **.01868** Mean e_1 (weighted) = **5.349**

TABLE V

Positive drop No. 15

Distance between cross hairs = 1.033 cm.
Temperature = 20° C.

	G (sec.)	F (sec.)	n	$e \times 10^{10}$	$e_1 \times 10^{10}$
	30.4	12.8	10	52.06	5.206
	30.5	17.9	8	41.61	5.200
	30.6	43.8	5	26.08	5.216
	30.2	85.9			
	30.5	85.9			
$G = $ **30.48**	30.7	86.4			
$V = 9010$	30.5	85.6	4	20.84	5.210
	30.7	86.2			
$F = 86.09$	30.5	86.2			
	—	86.4			
	30.2	2520.0	3	15.55	5.183

v_1 = **.04265** Mean e_1 (weighted) = **5.208**

e_1. This is illustrated by the observations shown in tables II., III., IV., V., VI. and VII. The drops shown in tables II. and III. were of almost exactly the same size, as is seen from the closeness of the values of the two velocities under gravity, and although the field strength was in one case double that in the other the values of e_1 obtained are almost

TABLE VI

Positive drop No. 16

Distance between cross hairs = 1.317 cm.
Temperature = 27.6° C.

	G (sec.)	F (sec.)	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
	24.61[4]	151.9			
	24.4	152.9			
$F = 152.9$	24.63	152.4	5	25.75	5.150
	24.6	153.5			
$V = 9075$	24.4	153.9			
$G = $ **24.57**	24.7	39.4	7	36.03	5.147
	24.8	29.2			
	24.6	28.6			
$F = 28.92$	24.50	28.9	8	41.07	5.134
	24.59	29.0			
	24.54	16.0			
	24.53	16.0	11	56.25	5.114
$F = 15.93$	—	15.8			

v_1 = **.05360** Mean e_1 (weighted) = **5.143**

TABLE VII

Negative drop No. 17

Distance between cross hairs = 1.305 cm.
Temperature = 26.8° C.

	G (sec.)	F (sec.)	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
	23.8	31.5			
$F = 31.33$	23.6	31.3	8	41.10	5.139
	23.4	31.2			
	23.7	43.8			
$G = $ **23.58**	23.7	43.6			
$V = 8975$	23.8	43.7	7	36.09	5.156
$F = 43.72$	23.5	43.4			
	23.2	43.4			
$F = 24.2$	23.5	24.2	9	46.29	5.144

v_1 = **.05534** Mean e_1 (weighted) = **5.145**

identical. Similarly tables VI. and VII. are inserted to show the consistency which could be attained in determining the values of e_1 so long as the drops used were of the same size. On the other hand, the series of tables II., IV., V. and VI. or III., IV., V. and VII. show conclusively that the value of e_1 obtained in this way diminishes as the velocity of the drop increases. This means of course that Stokes's law does not hold for these drops.

In order to find in just what way this law breaks down we made an extended series of observations upon drops the velocities of

[4] The readings carried to hundredths of a second were taken with a chronograph, the others with a stop watch. The mean G from the chronograph readings is 24.567, that from the stop-watch readings 24.583.

which varied in the extreme cases 360 fold. These velocities lay between the limits .0013 cm. and .47 cm. per second. Complete records of a few of these observations are given in tables VIII., IX., X. and XI.

On account of the obvious importance of obtaining accurate readings on the larger drops, for which Stokes's law should most nearly hold, the times of fall of such drops under gravity were taken with a chronograph with as great care as possible. Also wherever it was possible, the same drop was timed by both Mr. Fletcher and myself in order to eliminate the personal equation. The degree of precision which we attained can be judged from the readings recorded in the columns headed G in tables VIII., IX., X. and XI. It will be seen that we very seldom made a reading of the time interval involved in the passage of our star between the cross hairs which differed from the mean time interval by more than one twenty-fifth of a second. Furthermore, Mr. Fletcher's and my own mean times on a given drop generally differ from each other by less than one one-hundredth of a second.

All of the times recorded under F in these tables were taken with a stop watch for the reason that in view of the way in which v_1 and v_2 enter into formula (4) and also in view of the fact that F was in all these observations very much larger than G no increase in the accuracy of e_1 could be obtained by the use of a chronograph in the observations on v_2.

The volts were read just before and just after the observations on a given drop by dividing the bank of storage cells into eleven parts and reading the *PD* of each part by means of a 900 volts Kelvin and White electrostatic voltmeter which we calibrated with an accuracy of one tenth of one per cent. by comparing it with a Weston voltmeter which had been standardized at the Bureau of Standards.

The letter F before a reading means that it was taken by Fletcher, the letter M that it was taken by Millikan.

It will be seen from the tables that even in the case of the largest drops, which were charged with as many as 130 elementary units, the values of n are in every case unmistakably determined by the differences summarized at

TABLE VIII

Negative drop No. 20

Distance between cross hairs = 1.314 cm.
Temperature = 23.4° C.

	G (sec.)	F (sec.)	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
	M 84.87	114.7			
$V=8431$	" 14.88	114.8	11	56.14	5.104
	" 14.87	115.3			
$F=114.9$	" 14.90	64.2			
$G=$ **14.857**	" 14.85	64.8	12	61.20	5.100
$V=8428$	" 14.82	64.2			
$F=64.35$	" 14.84	64.2			
$V=8423$	" 14.84	117.0	11	56.12	5.102
$F=117.0$	" 14.84	117.0			
$v_1 =$ **.08843**				Mean $e_1 =$	**5.102**

TABLE IX

Negative drop No. 27

Distance between cross hairs = 1.317 cm.
Temperature = 25.2° C.

	(G sec.)	F (sec.)	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
$V=8793$	F 8.03	48.6	28	141.78	5.063
$F=99.35$	" 8.03	98.9	26	131.58	5.061
$V=8792$	" 8.08	99.8			
$F=67.05$	" 8.06	67.2	27	136.34	5.050
$V=8790$	" 7.96	66.9			
	" 7.98	32.7	30	151.69	
	M 7.96	32.6			
	" 8.04	27.6	31		
$F=32.66$	—	32.6			
$V=8788$	" 7.92	32.7	30	151.69	5.056
$G=$ **8.013**	—	32.7			
	" 8.02	32.7			
$F=24.67$	—	24.7			
$V=8786$	—	24.6	32	161.41	5.044
	" 8.06	24.7			
	Forced change with radium				
$V=8785$	" 8.03	50.5	28	141.20	5.043
$F=68.3$	—	68.2	27	136.17	5.043
$V=8784$	" 8.01	68.4			
$F=107.15$	—	107.2	26	131.05	5.040
$V=8782$	" 8.01	107.4			
$v_1 =$ **.16436**				Mean $e_1 =$	**5.050**

F's mean $G = 8.023$. M's mean $G = 8.007$.

Differences

	e_n		n		e_1	Prob. error
141.78 — 131.58 =	10.20	÷	2	=	5.10	1 per cent.
136.34 — 131.58 =	4.76	÷	1	=	4.76	2 per cent.
151.69 — 136.34 =	15.35	÷	3	=	5.12	2 per cent.
161.41 — 141.20 =	20.20	÷	4	=	5.05	1 per cent.
141.20 — 136.17 =	5.03	÷	1	=	5.03	2 per cent.

Weighted mean difference = 5.03.

TABLE X

Negative drop No. 29

Distance between cross hairs = 1.007 cm.
Temperature = 21.8° C.

	G sec.	F sec.	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
$V = .8845$	—	16.8	46	232.07	
	—	15.0			
$F = 15.07$	—	14.8	47	238.43	
$V = .8845$	—	15.4			
	—	18.5			
$F = 18.60$	—	18.7	45	227.21	
$V = 8844$	—	18.6			
	—	20.6	44	222.67	
	F 4.66	27.5			
	" 4.69	27.5			
	" 4.57	27.8			
	" 4.61	27.9			
	—	27.9			
$F = 27.73$	" 4.66	27.7			
$V = 8843$	" 4.56	27.6	42	212.70	5.064
	" 4.60	27.7			
	" 4.65	27.6			
	—	27.7			
	M 4.60	—			
	" 4.62	28.0			
	" 4.61	27.9			
	" 4.60	33.6			
	" 4.68	33.8			
$F = 33.75$	" 4.61	33.8			
$V = 8841$	—	33.7	41	207.33	5.057
	" 4.64	33.7			
	" 4.62	33.9			
$F = 42.55$	" 4.61	42.5			
$V = 8840$	" 4.61	42.6	40	202.28	5.057
	" 4.64	33.8			
$F = 34.05$	—	34.2			
$V = 8839$		34.2	41	207.30	5.055
	" 4.66	34.0			
	" 4.67	34.8			
	—	34.4	41		
$G = \mathbf{4.630}$	" 4.68	34.8			
	" 4.61	28.8	42		
$F = 34.67$	" 4.66	34.5			
$V = 8837$	—	34.8	41	206.86	5.045
	" 4.62	34.7			
Forced change with radium.					
$F = 59.50$	F 4.58	59.4			
$V = 8836$	" 4.63	59.6	39	196.75	5.045
	" 4.64	60.0			
$F = 44.1$	—	44.1			
$V = 8835$	" 4.64	44.0	40	201.69	5.041
	" 4.63	44.2			
Forced change with radium.					
$F = 219.3$	F 4.66	216.7			
$V = 8834$	" —	222.0	37	186.39	5.038
Forced change with radium.					
	F 4.64	35.0			
$F = 35.2$	" 4.60	35.2			
$V = 8833$	" 4.65	35.4	41	206.59	5.039
	" 4.65	35.2			
	" 4.67	44.8			
$F = 45.66$	—	45.2			
$V = 8831$	" 4.60	45.4	40	201.30	5.033
	—	45.4			
		45.5			
	—	35.6	41		
Forced change with radium.					
	—	19.1			
	—	19.6			
	—	19.2			
	—	19.6			
	—	19.5			
$F = 19.42$	—	19.4			
$V = 8829$	—	19.3			
	—	19.2	45	226.21	
	—	19.7			
	—	19.6			
	—	19.3			
	—	19.2			
	—	19.7			
	—	19.5			
Forced change with radium.					
	—	64.0			
$F = 63.45$	—	66.4			
$V = 8827$	—	63.0	39	196.12	
	—	63.4			
$F = 100.2$	—	100.0			
$V = 8826$	—	100.3	38	191.11	

$v = .2175$ Mean $e_1 = \mathbf{5.046}$

F's mean $G = 4.629$. M's mean $G = 4.632$.

Differences

	e_n		n		e_1	error
196.12 — 191.11 =	5.01	÷	1	=	5.01	1 per cent.
226.21 — 196.12 =	30.09	÷	6	=	5.02	1 per cent.
226.21 — 201.30 =	24.11	÷	5	=	4.98	2 per cent.
206.59 — 186.39 =	20.20	÷	4	=	5.04	1 per cent.
201.69 — 186.39 =	15.30	÷	3	=	5.10	1 per cent.

Mean difference (weighted) = 5.035.

the bottoms of the tables. In fact, in general, even with the largest drops the relative value[5] of e_1 can be determined with an accuracy of .5 per cent. from the differences alone. The accuracy is, of course, increased by dividing the values of e_n by n as soon as n has been found with certainty from the differences.

The readings shown in these tables are merely samples of the sort of observations which we took on between 100 and 200 drops between December, 1909, and May, 1910. The sort of consistency which we attained after we had learned how to control the evaporation of the drops, and after we had eliminated dust from the air, may be seen from table XII., which contains the final results of our observations upon all of the drops except three

[5] Since the same value of G is used in computing all of the e_ns the relative values of e_n or e_1 are practically independent of the error in G.

studied throughout a period of 47 consecutive days. The three drops which have been excluded all yielded values of e_1 from two to four per cent. too low to fall upon a smooth e_1v_1 curve like that shown in Fig. 1, which is the graph of the results contained in table XII. It is probable that these three drops corresponded not to single drops, but to two drops stuck together. Since we have never in all our study observed a drop which gave a value of e_1 appreciably above the curve of Fig. 1, or

TABLE XI

Negative drop No. 32

Distance between cross hairs = 1.003 cm.
Temperature = 23.2° C.

	G (sec.)	F (sec.)	n	$e_n \times 10^{10}$	$e_1 \times 10^{10}$
$F = 8.5$	—	8.7			
$V = 8577$	—	8.3	123	622.40	
	—	8.5			
Changed without radium.					
	M 2.44	28.4			
	—	28.7			
	—	28.7			
$F = 28.70$	" 2.46	28.4	104	524.25	5.040
$V = 8573$	" 2.54	29.0			
	" 2.46	29.0			
	" 2.45	28.8			
	" 2.43	28.6			
Change forced with radium.					
$G = $ **2.462**	" 2.44	15.7			
	" 2.48	15.7			
$F = 15.72$	—	15.7	111	558.78	5.034
$V = 8568$	—	15.7			
	—	15.8			
Change forced with radium.					
$F = 59.1$	—	59.1			
$V = 8565$	" 2.50	59.1	100	503.42	5.034
$F = 60.0$	—	59.8			
$V = 8563$	F 2.45	60.2	100	503.22	5.032
Change forced with radium.					
$F = 81.5$	—	81.0			
$V = 8561$	—	82.1	99	498.12	5.031
Change forced with radium.					
	" 2.44	19.0			
$F = 20.0$	" 2.50	20.1	108	543.41	5.032
$V = 8555$	" 2.42	20.0			
$v = .4074$		Mean e_1 (weighted) = 5.033			

F's mean $G = 2.452$. M's mean $G = 2.467$.

Differences

e_n	n	e_1	Prob. error
543.41 — 498.12 = 45.29 ÷	9	= 5.032	.5 per cent.
503.23 — 498.12 = 5.11 ÷	1	= 5.11	3.0 per cent.
558.78 — 503.42 = 55.36 ÷	11	= 5.035	.5 per cent.
558.78 — 524.25 = 34.53 ÷	7	= 4.94	3.0 per cent.

Mean difference (weighted) = 5.031.

and since further a sphere must have a higher rate of fall than a body of any other form whatever having the same mass and density, the hypothesis of binary drops to account for an occasional low value of e_1 is at least natural. After eliminating dust we found not more than one drop in ten which was irregular. The drop shown in table I. is perhaps the best illustration of the case under consideration which we have observed. It yields a value of e_1, which is four per cent. too low to fall on the curve of Fig. 1. This is as large a departure from this curve as we have thus far obtained.

§ 6. *The Correction of Stokes's Law.*—The procedure actually adopted for correcting Stokes's law will be detailed elsewhere. The end result is this. An equation of the following form is made to replace Stokes's equation (2):

$$X = 6\pi\mu a v \left(1 + A\frac{l}{a}\right)^{-1} \qquad (5)$$

$$v_1 = \frac{2}{9}\frac{ga^2(\sigma - \rho)}{\mu}\left\{1 + A\frac{l}{a}\right\}, \qquad (6)$$

in which a is the radius of the drop, l the mean free path of the gas molecule, and A an undetermined constant which we obtain from our observations. It turns out that A is identical to within the limits of observational error (not more than 1 or 2 per cent.) with the value deduced by Cunningham[6] from the kinetic theory considerations, provided the f of his formula[7] is made equal to zero. This means that the value of A given by our observations is .815. The values of a in tables XII. and XIII. are computed from (6), in which a is now the only unknown.

§ 7. *The Absolute Value of e.*—Using now (6) instead of (4) to combine with (1) and denoting by e the absolute value of the elementary charge and by e_1, as heretofore, the value of this charge as obtained from the use of the usual form of Stokes's law, *i. e.*, from (4) there results at once

$$e\left(1 + A\frac{l}{a}\right)^{\frac{3}{2}} = e_1. \qquad (7)$$

[6] *Proc. Roy. Soc.*, 83, p. 360.

[7] Cf. p. 361, *l. c.*

Table XIII. contains the values of e obtained from all of the observations recorded in table XII. except the first four and the last six. These are omitted not because their introduction would change the final value of e, for as a matter of fact this is not appreciably altered by including them, but solely because of the experimental errors involved in work upon either exceedingly slow or exceedingly fast drops. When the velocities are exceedingly slow residual convexion currents introduce errors, and when they are exceedingly fast the time determination becomes uncertain.

The final mean value of e is 4.9016×10^{-10}. The probable error computed from the number of observations shown in the last column and their average divergence should be about one tenth of one per cent. Since, however, the coefficient of viscosity of air is involved in the formula the accuracy with which e is known is limited by that which has been obtained in the measurement of this constant. After a prolonged and very careful study of all the data available on the viscosity of air I have chosen as the most probable value of μ at 15°.0001785. For reasons which will be detailed elsewhere it is thought that the error in this value is less than one half of one per cent.

It is most interesting that the agreement between Cunningham's rational formula and our experimental results is so perfect. How perfect it is may be seen graphically from Fig. 2, in which the curve is computed from 7 under the assumption of $e = 4.9016$ and our experimentally determined values of e are plotted about this curve, every observation contained in Table XII. being shown in the figure. Nevertheless, it is to be particularly emphasized that the correctness of our final value of the elementary electrical charge is completely independent of the correctness of any theory whatever as to the cause of the failure of Stokes's law for small drops. It is entirely possible that a series of experiments of this kind upon substances other than oil may lead to other values of A,

TABLE XII

No.	Velocity cm./sec.	Radius cm.	$e_1 \times 10^{10}$	Prob. Error.
1	.001315	.0000313	7.384	6.0
2	.001673	358	6.864	4.0
3	.001927	386	6.142	2.5
4	.006813	755	5.605	1.5
5	.01085	967	5.490	.5
6	.01107	979	5.496	.7
7	.01164	.0001004	5.483	.4
8	.01176	1006	5.482	.4
9	.01193	1016	5.458	.8
10	.01339	1084	5.448	.5
11	.01415	1109	5.448	.4
12	.01868	1281	5.349	.5
13	.02613	1521	5.293	.5
14	.03337	1730	5.257	.5
15	.04265	1954	5.208	.5
16	.05360	2205	5.148	.4
17	.05534	2234	5.145	.5
18	.06800	2481	5.143	.7
19	.07270	2562	5.139	.5
20	.08843	2815	5.102	.3
21	.09822	2985	5.107	.4
22	.1102	3166	5.065	.4
23	.1219	3344	5.042	.5
24	.1224	3329	5.096	.5
25	.1267	3393	5.061	.5
26	.15145	3712	5.027	.5
27	.1644	3876	5.050	.3
28	.2027	4297	4.989	.7
29	.2175	4447	5.046	.4
30	.3089	5315	4.980	1.0
31	.3969	6047	5.060	1.0
32	.4074	6104	5.033	1.0
33	.4735	6581	4.911	1.5

TABLE XIII

No.	Velocity cm./sec.	Radius cm.	$e_1 \times 10^{10}$	Prob. Error.	$e \times 10^{10}$	Per Cent. Error.
5	.01085	.0000967	5.490	.5	4.892	.20
6	.01107	979	5.496	.7	4.889	.26
7	.01164	.0001004	5.483	.4	4.903	.03
8	.01176	1006	5.483	.4	4.916	.28
9	.01193	1016	5.458	.8	4.891	.22
10	.01339	1084	5.448	.5	4.908	.10
11	.01415	1109	5.448	.4	4.921	.42
12	.01868	1281	5.349	.5	4.900	.03
13	.02613	1521	5.293	.5	4.910	.17
14	.03337	1730	5.257	.5	4.918	.34
15	.04265	1954	5.208	.5	4.913	.21
16	.05360	2205	5.143	.4	4.884	.36
17	.05534	2234	5.145	.5	4.885	.34
18	.06800	2481	5.143	.7	4.912	.21
19	.07270	2562	5.139	.5	4.913	.01
20	.08843	2815	5.102	.3	4.901	.01
21	.09822	2985	5.107	.4	4.915	.27
22	.1102	3166	5.065	.4	4.884	.36
23	.1219	3344	5.042	.5	4.882	.40
24	.1224	3329	5.096	.5	4.923	.44
25	.1267	3393	5.061	.5	4.894	.15
26	.15145	3712	5.027	.5	4.880	.44
27	.1644	3876	5.050	.3	4.903	.03

Final mean $e = 4.9016$

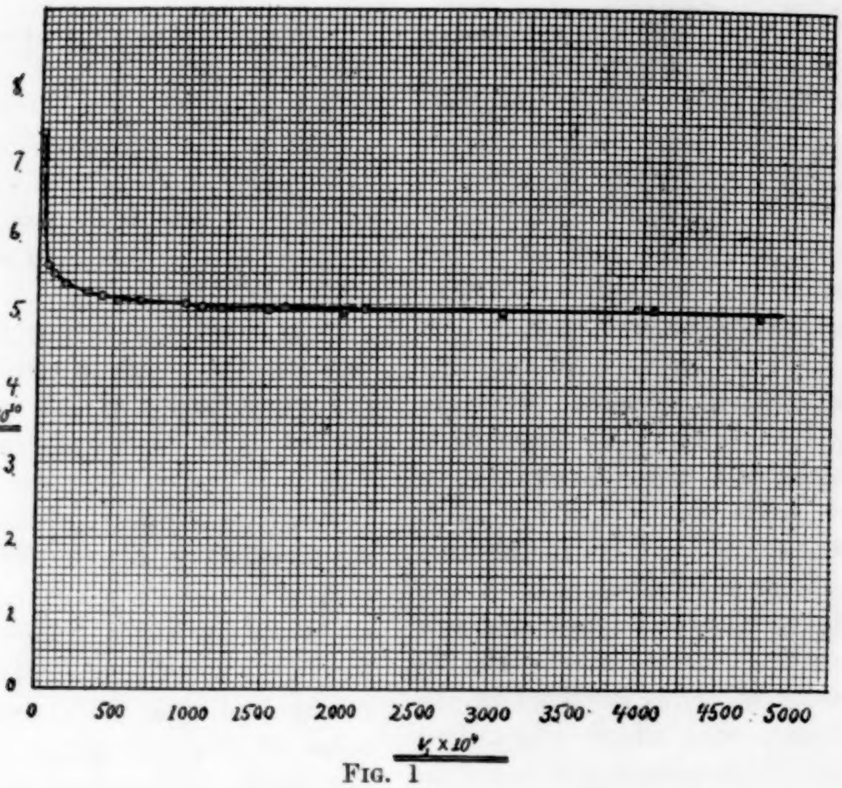

FIG. 1

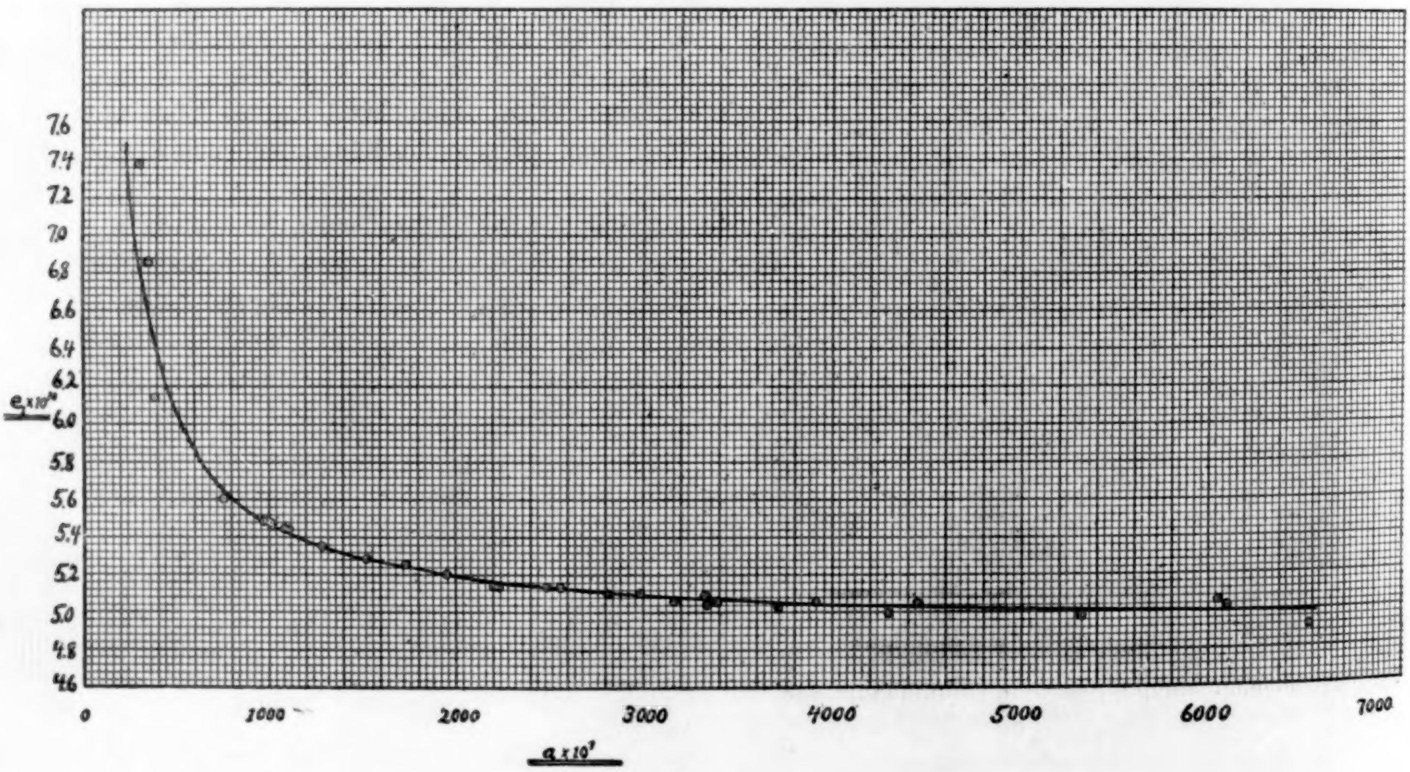

FIG. 2

but the value of *e* should in no way be affected thereby. It is of immense interest to know whether varying the mean free path by varying the pressure will affect the value of *A* in the way in which it ought according to Cunningham's theory, and we shall soon be in a position to settle this point and to make a further communication upon it.

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